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H. F. NEWALL, Esq., M.A., F.R.S., PRESIDENT, in the Chair.

Frederic Hermann Albert Alfred Buss, 2 Lansdowne Terrace, Grosvenor Square, Ashton-on-Mersey, near Manchester;
Arthur du Pré Denning, M.Sc., Ph.D., Birmingham University, and 18 Lightwoods Hill, Birmingham;
Herbert Shaw, A.R.C.S., Royal College of Science, South Kensington, and 6 Gowan Avenue, Fulham, S.W.; and
James Henry Worthington, Student in the University of Oxford, Bindon, Wellington, Somerset,

were balloted for and duly elected Fellows of the Society.

The following candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended :—

Captain Richard Algernon Craigie Daunt, D.S.O., Lynalta, Newtownards, Co. Down, Ireland (proposed by Rev. A. L. Cortie); and
Edgar Odell Lovett, Ph.D., Professor of Astronomy, Princeton University, New Jersey, U.S.A. (proposed by J. W. L. Glaisher).

One hundred and four presents were announced as having been received since the last meeting, including, amongst others :—

M. Jarry-Desloges, Observations des surfaces planétaires, fasc. 1, presented by the author; E. W. Maunder, The Astronomy of the Bible, presented by the author; C. H. F. Peters, Heliographic positions of Sun-spots, 1860–1870, edited by Professor Frost, pre-

sented by the Carnegie Institution; J. A. Repsold, Geschichte der Astronomischen Messwerkzeuge, presented by Mr. Franklin-Adams; E. B. H. Wade, Field method of determining longitudes by observations of the Moon, presented by the Egyptian Survey Department.

Astrographic Chart; 32 charts, presented by the Royal Observatory, Greenwich; 20 charts, from Algiers and Paris Observatories, presented by the French Government; and 20 charts, presented by the San Fernando Observatory.

Series of 36 collotype reproductions of photographs of the Milky Way, etc., presented by Professor E. E. Barnard; photograph of the Nebula in Orion (transparency) from negative taken by Professor Perrine with the Crossley reflector, presented by the Lick Observatory.

A suggested explanation of the ancient Jewish Calendar Dates in the Aramaic Papyri translated by Professor A. H. Sayce and Mr. A. E. Cowley. By E. B. Knobel.

The Aramaic papyri discovered at Assuan, on the site of the ancient Syene, which have been recently translated and published by Professor Sayce and Mr. Cowley, are of unique interest and importance owing to the duplicate dates given to each document. These documents cover a large part of the fifth century B.C., extending from B.C. 471, nine years only after the battle of Salamis, to B.C. 410. The papyri all relate to a Hebrew colony established at that period at Syene, and deal with rights of property, conveyance of land and buildings, marriage portions, and legal processes. They are all deeds most carefully drawn, signed, sealed, and witnessed, and they are dated according to both the Egyptian and Hebrew calendars, in the regnal years of the kings of Persia.

The Egyptian year and calendar are well understood. The year was a vague solar year, and consisted of 365 days without intercalation or correction, consequently the Julian date of the commencement of the Egyptian year recedes one day every four years. The year consisted of twelve months, each of thirty days, and five additional days, called *epagomenæ*, were added after the last month. There is consequently no difficulty with this calendar in determining the corresponding Julian date.

Very little, however, is known of the Jewish calendar in use at the period under consideration. The present reformed calendar dates only from the time of Hillel in the fourth century A.D., though it was probably not finally settled until after the fifth century. It is known that in olden times the year was a lunar year, and certain months, and ordinances connected with the months and seasons, are mentioned in the Old Testament. There is no mention of an intercalary month in the Bible, and it is not

known whether the correction to the solar year was applied in ancient times by the addition of one month in three years, or by the adding of ten or eleven days at the end of each year. No information appears to exist that there was anything like a settled Jewish calendar so far back as the fifth century B.C.

It is very generally stated that prior to the adoption of the reformed calendar the Jews employed the era of the Seleucidæ, the years of which were Julian of 365 days, but this could not have been the case at the period under discussion. Burnaby's work on the Jewish calendar gives little assistance in the present investigation.

Mr. Margoliouth—a high authority—writes: "No lists of pre-Christian Jewish dates reconciled with Egyptian or other dates are so far available to throw light on the exact form of the calendar used for the dating of the Aramaic documents published by Professor Sayce and Mr. Cowley. In the fifth century B.C. the Jewish calendar depended entirely on the observation of the Sun and the Moon, particularly the latter. The decisions must have been made by a central court, as was practically the case down to 359 A.D., so that great uncertainty would be caused in distant parts (such as Syene in Upper Egypt, to which the papyri belong) by the delay in transmitting the announcements.

"It is also uncertain whether the Jewish lunar year was in ancient times harmonised with the solar year by the addition of one month in three years, or by lengthening the last month in each year. The difficulties connected with the dates given in the recently published papyri may possibly have to be ascribed to the uncertainties mentioned."

Professor Schürer has discussed the subject in the *Theologische Literaturzeitung* for February 1907, in which he claims that the papyri confirm the fact that the Jews began their months with the *appearance* of the new moon, and further that they show that "it was far from the case that any definite system had been adopted."

Dr. Lidzbarski has also reviewed these papyri in the *Deutsche Literaturzeitung* for 1906, but his discussion is more particularly philological, and contributes little towards the question of the ancient calendar of the Jews.

The object of the present paper is to inquire whether more definite information on the subject cannot be derived from the Aramaic papyri themselves.

The dates of each papyrus, as given by the translators, are as follows. The figures in brackets indicate possible alternative dates according as a certain slanting mark in the writing is considered as forming part of the numeral or not. The present opinion is that it should do so, and that the higher number is the correct one, which I have accordingly adopted.*

A. On the 17th (18th?) of Elul, that is the 27th (28th?) day of Pachons, the 14th (15th?) year of Xerxes the king . . . .

* An exception may probably be made in the day of Thoth in B.

B. On the 18th (?) of Chisleu, that is the 6th (7th ?) day of Thoth, the 20th (21st ?) year (of Xerxes), the beginning of the reign when Artaxerxes the king ascended his throne . . . .

C. Mutilated as to the dates.

D. On the 21st Chisleu, that is the 1st of Mesore, the 6th year of Artaxerxes the king . . . .

E. On the 3rd of Chisleu, that is the 10th day of the month Mesore, the 19th year of Artaxerxes the king . . . .

F. On the 13th (14th ?) of Ab, that is the 19th day of Pachons, the 25th year of Artaxerxes the king . . . .

G. On the 26th (?) of Tishri . . . . the 6th (day) of the month Epiphi [the 25th year of Artaxerxes the king] . . . .

H. In the month Elul, that is Payni, the 3rd (4th ?) year of Darius the king.

J. On the 3rd of Chisleu, the 7th (8th ?) year, that is the 11th (12th ?) day of Thoth, the 7th (8th ?) year of Darius the king . . . .

K. On the 23rd (24th ?) of Shebat, the 13th year, that is the 8th (9th ?) day of Athyr, the 13th (14th ?) year of Darius the king . . . .

The dates definitely adopted from the translation are as follows:—

A.	15th year of	Xerxes,	28th Pachons	=	18th Elul.
B.	1st ,,	Artaxerxes,	6th Thoth	=	18th Chisleu.
E.	19th ,,	,,	10th Mesore	=	3rd Chisleu.
F.	25th ,,	,,	19th Pachons	=	14th Ab.
J.	8th ,,	Darius,	12th Thoth	=	3rd Chisleu.
K.	14th ,,	,,	9th Athyr	=	24th Shebat.

For the regnal years of the kings I have adopted the dates given by Ricard in his edition of Plutarch, thus:—

Cambyses,	1st year	B.C.	529
Smerdis (7 months),*	,, ,,		522
Darius Hystaspes,	,, ,,		521
Xerxes the Great,	,, ,,		485
Artabanus (7 months),	,, ,,		464
Artaxerxes Longimanus,	,, ,,		464
Xerxes II. (a month),	,, ,,		425
Sodgianus (7 months),	,, ,,		424
Darius II. (nothus)	,, ,,		423

The order of the Egyptian and Hebrew months is as follows:—

Egyptian Months.	Days.	Hebrew Months.	Days.
Thoth.	30	Tishri.	30
Phaophi.	30	Marheshvan.	29 or 30
Athyr.	30	Chisleu.	30 or 29
Choiak.	30	Tebeth.	29
Tybi.	30	Shebat.	30
Mechir.	30	Adar.	29

* Oppert.

Egyptian Months.	Days.	Hebrew Months.	Days.
Phamenoth.	30	Ve-Adar.	30
Pharmuthi	30	Nisan.	30
Pachons.	30	Iyyar.	29
Payni.	30	Sivan.	30
Epiphi.	30	Tammuz.	29
Mesore.	30	Ab.	30
5 Epagomenæ.		Elul.	29

In the papyri the Julian dates corresponding to the Egyptian dates are all known, and the problem, in the absence of all information on the subject, is to construct a reasonable and probable Jewish calendar which shall satisfy all the Jewish dates.

Fortunately the papyri E. and J. offer some assistance towards the solution of this difficult question. The Egyptian dates in Julian reckoning are as follows:—

E. B.C. 446 ... 10th Mesore = November 17,
J. B.C. 416 ... 12th Thoth = December 16,

but the Jewish date of both documents is the same, viz. 3rd Chisleu; consequently the period B.C. 446 November 17 to B.C. 416 December 16 should be an *exact number* of Jewish years.

It has been assumed by writers generally that the commencement of each month was determined by observation and announcement, and this was no doubt the common practice in the ecclesiastical year, which began with the 1st Nisan. The Jewish civil year, however, began unquestionably with the 1st Tishri; and with such a practical business people as the Jews, who, as we should infer from the papyri under consideration, enjoyed at this period a high state of civilisation, it is almost inconceivable that they should not have had in current use some calendar upon which they could base their business negotiations.

The reformed Jewish calendar is based upon the Lunar cycle of nineteen years—the so-called Metonic cycle—and it is not unreasonable to assume that this cycle was in use with the Jews long before the time of Hillel. With one exception, that of the French Revolution calendar, history does not record the *creation* of any calendar, but only the correction, reformation, or amendment of pre-existing calendars. Dr. Mahler pointed out in a paper read to the Oriental Congress of 1892 ("Das Kalenderwesen der Babylonier") that the Lunar cycle was in use by the Jews at Babylon before it was adopted by the Greeks, and that it was really of Babylonian origin. In discussing the order of the intercalary months, Al Biruni (A.D. 973–1048) (*The Chronology of Ancient Nations*) mentions one particular order which he says is preferred by the Jews, because they attribute its invention to the Babylonians.*

In this attempt to explain the Jewish calendar dates in the

* The period we are dealing with was only about sixty years after the Captivity, and it is reasonable to suppose that some of the colonists at Syene may have migrated from Babylon, as Professor Sayce particularly indicates Babylonish names among those mentioned in the documents.

papyri, it may therefore be justifiable to assume that the nineteen-year Lunar cycle was in current use. I have accordingly adopted the cycle with the same intercalations as are to be found in the present Jewish calendar, which is unchanged since the fourth century A.D., and upon this basis I have constructed a table for the whole period covered by the MSS., the intercalary months disposed according to Scaliger's rule, "ter, ter, bis, ter, ter, ter, bis." *

Normal Lunar Cycle.

No of Year.	Days.
1	354
2	354
3 Emb.	384
4	354
5	355
6 Emb.	384
7	354
8 Emb.	384
9	354
10	355
11 Emb.	384
12	354
13	354
14 Emb.	384
15	355
16	354
17 Emb.	384
18	354
19 Emb.	384

Applying this tentative calendar to the cases of papyri E., B.C. 446, and J., B.C. 416, it will be seen that there is *only one possible* position for those years in this Lunar cycle, and that B.C. 446 was the 17th and B.C. 416 the 9th year of that cycle, for this is the only position in which twelve intercalary years can be brought into a period of thirty years.

This gives coincidence between the number of days from B.C. 446 November 17 to B.C. 416 December 16, and the number of days in thirty Jewish years beginning with cycle No. 17 and ending with cycle No. 8 inclusive. On any other calculation there would be a difference of a month, and both deeds could not be dated in the same month Chisleu.

B.C. 446 Nov. 17 to B.C. 416 Dec. 15 inclusive = 10,987 days
30 Jewish years, cycle No. 17 to cycle No. 8 inclusive = 10,986 „

* In the old Chinese and Japanese calendar the intercalary months are disposed in this order.

It should be mentioned in explanation that were Dr. Mahler's Babylonian cycle employed, then B.C. 446 would be the 6th and B.C. 416 the 17th year of that particular cycle. So again in the cycle which Al Biruni says was preferred by the Jews, B.C. 446 would be the 14th and B.C. 416 the 6th year.

The table appended to this paper of the 1st day of Tishri from B.C. 523 to B.C. 406 has been constructed in the following manner :— The Lunar cycle numbers are laid down for the whole period from the numbers fixed for B.C. 446 and B.C. 416, and the days of each Jewish year appended. The Jewish astronomical computation of the length of a Lunar cycle is 6939 days 16 hours and 595 chalakim.* As the table extends over six cycles, an empirical correction had to be made making some cycles 6940 days, so that the mean length of the six cycles is 6939 days 16 hours.†

It was then necessary to find reliable data for determining the 1st day of Tishri for any year, so that a calendar could be constructed so far on a sound basis. Fortunately this was afforded by the most interesting discovery a few years ago by Father Strassmeier of a Babylonian tablet recording a partial lunar eclipse at Babylon in the 7th year of Cambyses. This cuneiform tablet has been fully translated and discussed by Oppert (*Zeitschrift für Assyriologie,* vol. vi.). It has an entirely unique interest, as it is an account of one of the eclipses recorded by Ptolemy in the Almagest.

Ptolemy states that the eclipse occurred in the 7th year of Cambyses, in the 225th year of Nabonassar, on the night of the 17th and 18th of the Egyptian month Phamenoth. Strassmeier's Babylonian tablet gives the date as the 7th year of Cambyses, on the 14th day of the Jewish month Tammuz. The Julian date of the eclipse is determined by Pingré and Oppolzer as B.C. 523 July 16.

From this it is easy to calculate the date of the 1st Tishri as September 29 ; and as the 7th year of Cambyses is well identified as B.C. 523, the table appended is calculated entirely from this date—from B.C. 523 to B.C. 406. It gives the Year B.C.—Julian period — No. in Lunar cycle—Days in each year — Julian date of 1st Thoth—Julian date of the 1st Tishri ; and Greenwich Mean Time of New Moon nearest to the 1st Tishri, taken from Ginzel's *Handbuch der Mathematischen und Technischen Chronologie.*

In considering the coincidence of Julian and Jewish dates, it should be remembered that the Jewish day is defined in Genesis : "And there was evening and there was morning, one day,"—that is to say, the day begins at 6 o'clock in the evening and goes on to 6 o'clock the next evening, consequently one Jewish date extends over part of two Julian days.

* 1080 chalakim equal 1 hour.

† I have avoided complicating the question by reference to the "regular," "deficient," and "abundant" years, as exactitude is impossible, and it seemed sufficient to secure the correctness of the mean Lunar cycle.

Discussion of Dates.

A.

15th year of Xerxes	... B.C. 471 ...	1st Thoth	... Dec. 19
		28th Pachons	... Sept. 12
		1st Tishri	... Sept. 24
		18th Elul	... Sept. 12

B.

1st year of Artaxerxes	... B.C. 464 ...	1st Thoth	... Dec. 17
		6th Thoth	... Dec. 22
		1st Tishri	... Oct. 6
		18th Chisleu	... Dec. 21

C.

This papyrus is too much injured for the dates to be deciphered. The authors state that it is written by the same scribe as D., and that there is strong evidence for considering both C. and D. as of the same date.

D.

The MS. states: "On the 21st Chisleu, that is the 1st Mesore, the 6th year of Artaxerxes the king." By no possibility can these dates—21st Chisleu and 1st Mesore—be harmonised. But there is a crease in the papyrus just before the words "1 Mesore," and in this crease there is an indication of a character which cannot be deciphered until the crease is flattened out. It is probable that the Egyptian date has not been correctly deciphered. Mesore is the last month of the Egyptian year, and it is followed by the five Epagomenæ, which were kept as feast days. The question may be asked, whether in dating deeds such as those under consideration the five Epagomenæ were not treated as continuous dates of the previous month, Mesore? Dr. Budge informs me that he has no experience of such a case, but he sees no reason why it should not be suggested. I venture to hazard the suggestion that the first Epagomene was designated as the 31st Mesore. Upon this pure assumption we should have, as the best that can be done for D.,—

B.C. 460 ...	1st Thoth	... Dec. 16	
	31st Mesore	... Dec. 11	
	1st Tishri	... Sept. 21	
	21st Chisleu	... Dec. 9	

E

19th year of Artaxerxes	... B.C. 446 ...	1st Thoth	... Dec. 13
		10th Mesore	... Nov. 17
		1st Tishri	... Sept. 17
		3rd Chisleu	... Nov. 17

F.

25th year of Artaxerxes ...	B.C. 440 ...	1st Thoth	... Dec. 11
		19th Pachons	... Aug. 26
		1st Tishri	... Oct. 10
		14th Ab	... Aug. 25

G.

The papyrus is very mutilated. The dates 26th Tishri and 6th Epiphi are fairly certain, but the regnal year of Artaxerxes is conjecture. The authors state that the date of this deed cannot be earlier than 446, and hardly later than 440. We have to find coincidence between 6th Epiphi and 26th Tishri. The table gives the following dates:—

B.C. 446	6th Epiphi ... Oct. 14	26th Tishri ... Oct. 12
445	,, 13	,, 30
444	,, 13	,, 19
443	,, 13	Nov. 7
442	,, 13	Oct. 28
441	,, 12	,, 16
440	,, 12	Nov. 4

From this it is probable that the year is B.C. 446, and this conclusion is supported by the fact that the scribe of G. is also the scribe of E., which is clearly B.C. 446. The regnal year would thus be the 19th of Artaxerxes.

H.

The papyrus states, "in the month Elul, that is Payni, the 3rd (4th?) year of Darius."

3rd year of Darius	Payni	began	Sept. 2	ended	Oct. 1
B.C. 421	Elul	,,	Sept. 11	,,	Oct. 9
4th year of Darius	Payni	began	Sept. 2	ended	Oct. 1
B.C. 420	Elul	,,	Aug. 31	,,	Sept. 28

Clearly the 4th year of Darius, B.C. 420, suits the case best.

J.

8th year of Darius ...	B.C. 416 ...	1st Thoth	... Dec. 5
		12th Thoth	... Dec. 16
		1st Tishri	... Oct. 15
		3rd Chisleu	... Dec. 15

K.

14th year of Darius ...	B.C. 410 ...	1st Thoth	... Dec. 4	... B.C. 411
		9th Athyr	... Feb. 10	... B.C. 410
		1st Tishri	... Sept. 20	... B.C. 411
		24th Shebat	... Feb. 8	... B.C. 410

The final results are as follows:—

		Julian Date from Egyptian.	Computed Date from Table.
A.		Sept. 12	Sept. 12
B.		Dec. 22	Dec. 21
C.	Mutilated.		
D.	Uncertain.	Dec. 11?	Dec. 9?
E.		Nov. 17	Nov. 17
F.		Aug. 26	Aug. 25
G.		Oct. 14	Oct. 12
H.	B.C. 420		
J.		Dec. 16	Dec. 15
K.		Feb. 10	Feb. 8

The above results are too near coincidence to be fortuitous, and, so far as the civil year is concerned, they refute the opinion that the commencement of the month was determined by the appearance of the new moon.

Two conclusions from the foregoing investigation may be safely hazarded: first, that the Lunar cycle of 19 years was in use in the Jewish calendar at this remote period, which, as Professor Sayce says, was little more than a century after the grandfathers and great-grandfathers of the parties mentioned in the papyri had fled into Egypt with Jeremiah; and secondly, that the order of intercalation at that time was not dissimilar to that in use to-day.

In drawing any conclusions, one may put aside possible errors of the scribe. It is highly improbable that in the first line of original and important deeds like these papyri the scribe would make such errors as would be common in copies.

These deductions do not harmonise with the views of the late distinguished chronologist M. Oppert. It may be assumed that what was current with the Jews at Babylon during the Captivity would have been continued by them in their subsequent migration. M. Oppert states that the apparition of the crescent moon signalised the commencement of the month, and in a paper "Sur l'ancien Calendrier Perse," * he claims to have proved that the Babylonians had no fixed system for their calendar until after the year B.C. 367; that prior to that period the 19-year cycle was in use, but the intercalary months were inserted without any order, and solely on astrological grounds; and that it was the Greek influence which gave to Babylon a fixed system, assigning to each year of the cycle its particular character, whether common or embolismic, and he denies the correctness of Dr. Mahler's conclusions.

This view can hardly be sustained, for in making the Babylonian date B.C. 523, 14th Tammuz, the basis of the appended table, it is most improbable that we should arrive at such coincidence of the Egyptian and Jewish dates of the papyri if there had been no fixed system at all. The table connects in a systematic manner

* Oriental Congress, 1897. In this paper he calculates October 6th as the 1st day of Tishri, B.C. 521, as it is found in the present table.

Babylonian dates with the dates used by the Jews at Syene over a century later; and, notwithstanding M. Oppert's characteristic remark that "on fait l'histoire avec les livres historiques et non pas avec les éclipses," the rock upon which this investigation is built is the lunar eclipse at Babylon in the 7th year of Cambyses.

Table of the 1st Tishri from B.C. 523 *to* B.C. 406.

Year B.C.	Julian Period.	Lunar Cycle.	Days.	1st Thoth.	1st Tishri.	G.M.T. New ☾.
523	4191	16	354	Jan. 1	Sept. 29	Sept. 27·57
522	2	17	384		Sept. 18	17·05
521 β	3	18	354	Dec. 31	Oct. 6	5·13
520	4	19	384		Sept. 25	24·80
519	5	1	354		Oct. 14	13·85
518	6	2	354		Oct. 3	3·27
517 β	7	3	384	Dec. 30	Sept. 21	21·41
516	8	4	354		Oct. 10	10·16
515	9	5	355		Sept. 29	29·17
514	4200	6	384		Sept. 19	18·46
513 β	1	7	354	Dec. 29	Oct. 7	6·47
512	2	8	384		Sept. 26	26·11
511	3	9	354		Oct. 15	15·18
510	4	10	355		Oct. 4	4·77
509 β	5	11	384	Dec. 28	Sept. 23	23·13
508	6	12	354		Oct. 12	11·95
507	7	13	354		Oct. 1	30·93
506	8	14	384		Sept. 20	20·02
505 β	9	15	355	Dec. 27	Oct. 8	7·91
504	4210	16	354		Sept. 28	27·41
503	1	17	384		Sept. 17	17·07
502	2	18	354		Oct. 6	6·15
501 β	3	19	384	Dec. 26	Sept. 24	24·69
500	4	1	355		Oct. 13	13·61
499	5	2	354		Oct. 3	2·72
498	6	3	384		Sept. 22	21·72
497 β	7	4	354	Dec. 25	Oct. 10	9·50
496	8	5	355		Sept. 29	28·81
495	9	6	384		Sept. 19	18·38
494	4220	7	354		Oct. 8	7·47
493 β	1	8	384	Dec. 24	Sept. 26	26·10
492	2	9	354		Oct. 15	15·12
491	3	10	355		Oct. 4	4·45
490	4	11	384		Sept. 24	23·51
489 β	5	12	354	Dec. 23	Oct. 12	11·25
488	6	13	354		Oct. 1	30·34
487	7	14	384		Sept. 20	Sept. 19·72

Year B.C.	Julian Period.	Lunar Cycle.	Days.	1st Thoth.	1st Tishri.	G.M.T. New ☾.
486	4228	15	355	Dec. 23	Oct. 9	Sept. 8·77
485 β	9	16	354	Dec. 22	Sept. 28	27·42
484	4230	17	384		Sept. 17	17·06
483	1	18	354		Oct. 6	6·03
482	2	19	384		Sept. 25	25·26
481 β	3	1	355	Dec. 21	Oct. 13	13·04
480	4	2	354		Oct. 3	2·03
479	5	3	384		Sept. 22	21·21
478	6	4	354		Oct. 11	10·15
477 β	7	5	355	Dec. 20	Sept. 29	28 72
476	8	6	384		Sept. 19	18·41
475	9	7	354		Oct. 8	7·47
474	4240	8	384		Sept. 27	26·91
473 β	1	9	354	Dec. 19	Oct. 15	14·78
472	2	10	355		Oct. 4	3·81
471	3	11	384		Sept. 24	22·83
470	4	12	354		Oct. 13	11·67
469 β	5	13	354	Dec. 18	Oct. 1	30·08
468	6	14	384		Sept. 20	19·71
467	7	15	355		Oct. 9	8·78
466	8	16	354		Sept. 29	28·40
465 β	9	17	384	Dec. 17	Sept. 17	16·77
464	4250	18	354		Oct. 6	5·59
463	1	19	384		Sept. 25	24·59
462	2	1	354		Oct. 14	13·35
461 β	3	2	354	Dec. 16	Oct. 2	1·54
460	4	3	384		Sept. 21	21·02
459	5	4	354		Oct. 10	10·09
458	6	5	355		Sept. 29	29·76
457 β	7	6	384	Dec. 15	Sept. 18	18·33
456	8	7	354		Oct. 7	7·25
455	9	8	384		Sept. 26	26·39
454	4260	9	354		Oct. 15	15·15
453 β	1	10	355	Dec. 14	Oct. 3	3·15
452	2	11	384		Sept. 23	22·44
451	3	12	354		Oct. 12	11·44
450	4	13	354		Oct. 1	1·08
449 β	5	14	384	Dec. 13	Sept. 19	19·72
448	6	15	355		Oct. 8	8·75
447	7	16	354		Sept. 28	28·11
446	8	17	384		Sept. 17	17·18
445 β	9	18	354	Dec. 12	Oct. 5	4·91
444	4270	19	384		Sept. 24	23·99
443	1	1	355		Oct. 13	Sept. 12·89

Year B.C.	Julian Period.	Lunar Cycle.	Days.	1st Thoth.	1st Tishri.	G.M.T. New ☾.
442	4272	2	354	Dec. 12	Oct. 3	Sept. 2·38
441 β	3	3	384	Dec. 11	Sept. 21	21·04
440	4	4	354		Oct. 10	10·12
439	5	5	355		Sept. 29	29·66
438	6	6	384		Sept. 19	18·93
437 β	7	7	354	Dec. 10	Oct. 7	6·72
436	8	8	384		Sept. 26	25·70
435	9	9	354		Oct. 15	14·49
434	4280	10	355		Oct. 4	3·78
433 β	1	11	384	Dec. 9	Sept. 23	22·35
432	2	12	354		Oct. 12	11·44
431	3	13	354		Oct. 1	1·07
430	4	14	384		Sept. 20	20·55
429 β	5	15	355	Dec. 8	Oct. 8	8·43
428	6	16	354		Sept. 28	27·48
427	7	17	384		Sept. 17	16·48
426	8	18	354		Oct. 6	5·31
425 β	9	19	384	Dec. 7	Sept. 24	23·70
424	4290	1	355		Oct. 13	12·74
423	1	2	354		Oct. 3	2·39
422	2	3	384		Sept. 22	22·03
421 β	3	4	354	Dec. 6	Oct. 10	10·01
420	4	5	355		Sept. 29	29·24
419	5	6	384		Sept. 19	18·26
418	6	7	354		Oct. 8	7·02
417 β	7	8	384	Dec. 5	Sept. 26	25·18
416	8	9	354		Oct. 15	14·12
415	9	10	355		Oct. 4	3·70
414	4300	11	384		Sept. 24	23·38
413 β	1	12	354	Dec. 4	Oct. 12	11·43
412	2	13	354		Oct. 1	30·45
411	3	14	384		Sept. 20	20·05
410	4	15	355		Oct. 9	8·81
409 β	5	16	354	Dec. 3	Sept. 28	26·80
408	6	17	384		Sept. 17	16·07
407	7	18	354		Oct. 6	5·04
406	8	19	384		Sept. 25	Sept. 24·67

32 *Tavistock Square, London, W.C.:*
1908 *March* 11.

Observations of Saturn's Ring at the time of its Disappearance in 1907, made with the 40-in. Refractor of the Yerkes Observatory. By E. E. Barnard. (Plates 9, 10.)

The position of Saturn for observation of the phenomena connected with the disappearance of the ring has been unusually favourable this year, though the south declination (5°) of the planet was somewhat unfortunate for northern observers.

At the reappearance of the ring in October 1891, Saturn rose only about two hours before the Sun, and the resulting low position and the approach of daylight made it very unfavourable for the observation of such a delicate thing as the ring when it is placed edge on towards us. At that time, with the 36-in. and 12-in. refractors of the Lick Observatory, and under such conditions, I was unable to see the ring (*M.N.* for April 1892, vol. lii. p. 419).

According to Professor Hermann Struve, the following table represents the dates of the present disappearances and reappearances of the ring of Saturn.*

1907.	Apr. 17.	Disappearance.	The Earth in the plane of the rings.
,,	July 26.	Reappearance.	The Sun in the plane of the rings.
,,	Oct. 4.	Disappearance.	The Earth in the plane of the rings.
1908.	Jan. 7.	Reappearance.	The Earth in the plane of the rings.

On account of the position of Saturn in the direction of the Sun, the disappearance of the ring in April was invisible from the Earth. The reappearance in January next will be favourable for observation.

The present paper deals with the reappearance and disappearance of the ring in July and October of this year (1907). The times are central standard time, 6^h slow of G.M.T.

The unusually bad spring weather prevented early observations with the 40-in. telescope. The planet was observed, however, as frequently as the weather and other circumstances would permit. It was supposed the "reappearance" of the ring, when the Sun passed through its plane on or about July 26, would be a definite phenomenon, and that the time of this reappearance could be determined with some sort of precision. But the reappearance was a remarkably gradual phenomenon, and there was no possible means of telling when it occurred. The ring simply very slowly and gradually got brighter, and for several days it was impossible to tell that any change had taken place; and then it became bright and almost linear. It was not, however, at this time that the greatest interest lay. It was sometime previous to the reappearance of the ring that the most important phenomena were visible.

When the planet was examined on July 2 the entire surface of the ring was easily seen, though the Sun was not then shining on its visible surface. Where it was projected on the sky, the ring

* Publications of the Astronomical Society of the Pacific, No. 114, June 10, 1907.

appeared as a greyish hazy or nebulous strip, which was not well defined under the best conditions. It was about 1·5 times as broad as the trace of the ring on the ball. Nothing could be seen of the sunlit edge of the ring, which must have been too thin to be visible. There were two nebulous condensations of greater brightness on the ring at each side of the planet. These were quite conspicuous, but were ill defined and nebulous, and of a pale grey colour. The ring and condensations were so pronounced that they were strongly visible at $15^h\ 50^m$ (36^m before sunrise), when the approaching daylight was very bright. They were still visible as late as $16^h\ 2^m$ (24^m before sunrise), and, though faint, could have been followed a little later yet. Rough measures were made of the distances of these condensations on the preceding side of the planet from the preceding limb.

The Centre of the most Distant Spot from the Preceding Limb.	The Centre of the Nearer Spot from the Preceding Limb.
7″·7(1) [7″·6]	2″·9(1) [2″·8]

One setting was made for the extreme diameter of the ring, and this gave 40″·9 [40″·0];* while two settings gave 18″·85 for the equatorial diameter of the planet.

At $15^h\ 4^m$ a faint satellite (Enceladus) following was visible on the south side of the ring, close to the second condensation on that side. The satellite and condensation were of the same brightness, which will give some idea of the brightness of the ring and condensations. The trace of the ring across the ball was not black. The seeing was not good enough to tell whether part of this trace was due to the shadow of the ring, which in all probability at that time did not reach to the ball. The width of this trace was measured by setting the wires so that their outer edges were separated by the width of the trace—a correction afterwards being applied for the thickness of the wires = 0″·10. The width was 0″·62(1) at $14^h\ 50^m$. The ring, as projected on the sky, seemed to be a little south of the trace.

Position of the trace of the ring on the ball at $15^h\ 30^m$:—

From South Limb.	From North Limb.
8″·83(3) [8″·63]	7″·84(3) [7″·66]

This trace was without any visible irregularities.

July 5. $15^h\ 0^m$. The condensations were again visible. The nearer condensations were estimated to be $1-\frac{1}{2}$ times as bright as the distant ones. One setting gave for the width of the trace of the ring on the ball 0″·70. I could see nothing of the sunlit edge of the ring.

The seeing was very poor, and the sky clouded a few minutes later and did not again clear.

* The square brackets throughout this paper mean that the enclosed values have been reduced to the mean distance of Saturn from the Sun. The numbers in parentheses are the number of settings of the micrometer wires. The times are central standard times, $6^h\ 0^m$ slow of Greenwich.

July 6. $14^h\ 30^m$. The ring was rather faint—very much fainter, apparently, than on July 2, and perhaps somewhat narrower.

Width of trace on ball 0″·62(2).

Position of the trace :—

From South Limb.	From North Limb.
8″·79(3) [8″·53]	8″·02(3) [7″·79]

There was a faint dark belt south of the trace of the ring, 6″·6(1) from the south limb. The condensations on the ring were visible as before. Nothing could be seen of the sunlit edge of the ring.

July 12. $14^h\ 10^m$. The feebly luminous ring was visible as a thin nebulous strip on each side of the planet. Seeing very bad. There was a satellite preceding and another (Enceladus) following the ring. When best seen, the ring was about midway in brightness between these satellites. It was very thin and of a pale nebulous colour. The sunlit edge could not be seen. As late as $15^h\ 35^m$ (57^m before sunrise) the ring was still distinctly visible.

July 23. $13^h\ 50^m$. Sky thick. Seeing very bad. The ring was dimly visible as a faint thin line. The sunlit edge could not be seen.

July 24. Full moon. Seeing poor. $14^h\ 20^m$. The full extent of the ring was visible as a narrow, almost thread-like strip. It was faint and nebulous, without any irregularities. A faint satellite (Enceladus) was close following the following edge of the ring; when best seen, the ring was perhaps one magnitude less bright than this satellite. There was no appearance of direct sunlight falling on the ring or its edge. The following ansa seemed to be the most distinct—perhaps a little brighter.

July 25. Full moon. $15^h\ 10^m$. The ring fairly well seen as a faint nebulous strip. $15^h\ 15^m$. There was a small satellite (Enceladus) preceding the preceding end of the ring; it was $1-\frac{1}{2}$ magnitude brighter than the ring, which was of a faint grey colour. Seeing fair. With an occulter in the eyepiece the ring was quite easily visible. There was no appearance of direct sunlight, however, but the same pale illumination seen previously. There did not seem to be any irregularities on the ring, which was very narrow, and quite distinct when best seen. The trace of the ring on the ball was not black. At $15^h\ 45^m$ ($1^h\ 0^m$ before sunrise) the ring could still be fairly well seen.

July 26. $15^h\ 0^m$. Seeing fair. Slight haze. Bright moonlight. The ring was a little brighter, but whether due to less moonlight or not, it was not possible to tell. A small satellite (Enceladus) near and preceding the preceding end of the ring was but little brighter than the ring, perhaps as much as half a magnitude.

$15^h\ 15^m$. The ring was decidedly brighter than on the 25th. This was perhaps due to the direct sunlight shining on it. It was very thin. A faint satellite (Tethys) followed the ball, and

was a little south of the ring. It was but a little brighter than the ring near it.

Width of trace of ring on ball = 0″·45(4).

15h 30m. The small satellite (Tethys) following was much brighter than the ring near it—perhaps one magnitude brighter. The ring on the sky was broader than the trace.

A power of 700 was then applied. With this the ring was pale and nebulous. The illumination did not then look like direct sunlight.

Position of trace of ring on ball at 15h 45m—

From South Limb.	From North Limb.
8″·77(2) [8″·22]	8″·23(2) [7″·72]

The thickness of the trace was 0″·52(4).

16h 0m (45m before sunrise). The ring was still easily seen. It was but little less bright than the satellite (Enceladus) preceding.

16h 2m. The ring was still faintly visible. The sky was white from daylight.

July 28. 11h 40m. The ring was bright, but much less bright than the ball of the planet. The illumination looked like true sunlight. The projection of the ring on the sky was south of the projection on the ball.

14h 30m. Thickness of trace on ball 0″·44(4). The illumination of the ring did not look like direct sunlight. It was a pale grey nebulous light.

15h 35m. The ring could not be traced up to the ball, but was discontinuous for some few seconds.

15h 45m. The following ansa seemed to be a little thicker than the preceding. A very faint belt on the ball was seen north. The ring on the sky was about twice as thick as its trace on the ball.

August 6. 13h 45m. Trace of ring on ball :—

From South Limb.	From North Limb.
8″·68(3) [8″·04]	8″·37(3) [7″·89]

Measures were very poor—seeing very bad.

August 11. 12h 40m. Position of trace :—

From North Limb.	From South Limb.
8″·25(3) [7″·59]	9″·37(3) [8″·63]

15h 0m. The ring was much less bright than the ball. It looked like a bar of light on each side of the planet. Seeing very poor.

September 1. 11h 5m. A faint belt was visible on each side of the trace of the ring.

September 3. 13h 0m. The trace on ball seemed to be thinner than usual, and did not seem very distinct. When best seen the ring was clean cut, like a bar sharply pointed at the ends. The thickness of the ring on the sky was roughly 0″·67(1).

September 8. 10h 45m. The trace of the ring on the ball was

occasionally seen. It was very narrow. The ring was bright and narrow.

September 10. $12^h\ 5^m$. The trace of the ring was very faint and narrow. The planet was occasionally well seen, but the trace on the ball could scarcely be made out.

October 1. $11^h\ 6^m$. Seeing bad. The ring was of a pale ashy colour and comparatively faint. It was not very thin, and looked like a nebulous strip.

October 4. $8^h\ 30^m$. The ring was entirely invisible, with magnifying powers of 460 and 700.

$8^h\ 40^m$. Feeble traces of the ring following the planet were suspected, but not certain. The presence of Titan near interfered. A satellite preceding the planet prevented any chance of seeing the ring on that side. There was a faint satellite (Mimas) following, which seemed to be on the ring. Seeing poor.

$9^h\ 10^m$. With an occulter in the field, in the moments of best seeing, feeble traces of the ring were visible following.

$9^h\ 25^m$. With occulter, the following part of the ring was feebly glimpsed, but it was very difficult.

At $9^h\ 45^m$ a hexagonal diaphragm was put on over the O G. This collected the stray light into six rays, leaving clearer sky between. An occulter was used in the eyepiece. With this, very feeble traces of the ring could be seen following, but not near the planet. It could also be seen feebly once in a while preceding. In moments of steadiness, the trace of the ring or shadow of the ring on the globe could be seen. But this would have been easy if the seeing had been good.

$10^h\ 0^m$. The seeing had got too bad to do anything with the planet.

$11^h\ 40^m$. The diaphragm removed, occulter on. The seeing very bad; could see nothing of the ring in the steadiest moments.

October 5. $10^h\ 0^m$. A few minutes' opportunity was offered to observe Saturn with the 40-in. The seeing was fair, and I think better than on the 4th. The ring was faintly visible with the occulter. It seemed to be decidedly brighter than on the 4th. It was very slender and faint. A satellite was close preceding the planet. The shadow of the ring on the ball was sharply marked, but the seeing was not good enough to show it black. The ring was much fainter than a faint satellite (Mimas) near its following end.

October 6. $9^h\ 25^m$. The seeing was extremely bad, with a very high wind.

$10^h\ 30^m$. Seeing very bad, but by glimpses with the occulter the ring could be seen occasionally very feebly. Even with good seeing the ring would have been faint.

$10^h\ 50^m$. In moments of steadiness, fairly good glimpses of the ring were had. It was certainly brighter than on the 4th, and perhaps somewhat brighter than on the 5th. The shadow on the ball could also be seen.

The planet was examined later, but the seeing was too bad for any observations of value.

October 8. $9^h\ 0^m$. The ring could only be seen by occultation of the planet. It was very faint and narrow. The south belt across the planet was far more conspicuous than the north one, which was rather difficult.

Position of the shadow on the ball. $9^h\ 10^m$:—

From South Limb.	From North Limb.
8″·92(3) [8″·08]	8″·98(3) [8″·14]

Very uncertain—the planet very faint in haze.

$11^h\ 0^m$. I could not see anything of the ring with Saturn uncovered in the field, but with occulter I could see it at both sides of the planet very easily, though it was faint. It was straight and very narrow, with no irregularities. A faint satellite at the following side interfered. The ring was easier than at the previous observations since its disappearance. The south belt was very diffused to the south, while the north belt was diffused to the north. The equatorial space between these two belts was the brightest region of the planet.

$11^h\ 25^m$. Thickness of shadow on ball was 0″·69(2). Distance of south belt from shadow = 2″·20(2). A satellite following half way to end of ring was very much brighter than the ring.

October 13. $8^h\ 25^m$. The ring could be seen with Saturn uncovered in the field. It was thin and rather faint. A satellite following and another preceding.

Micrometer positions on the ball :—

South belt from south limb	6″·77(2)	South belt from north limb	10″·94(2)
Shadow from south limb	8·81(2)	Shadow from north limb	8·25(2)
North belt from south limb	10·64(2)	North belt from north limb	7·30(2)

There was a faint broad shading parallel to the equator in the northern hemisphere. The southern edge of this was 13″·9(2) from the S. limb, and 3″·8(2) from the N. limb. The seeing was very bad. This last marking was a very faint and diffused belt. Though the ring was faint, it could be distinctly seen with 460 diameters, with Saturn uncovered in the field, while with the occulter it was seen quite easily on both sides.

$11^h\ 0^m$. The ring was easily visible without occultation of the planet. it was very thin, but by occultation it was quite conspicuous. There were two regions of greater brightness on both the preceding and following ansa [the condensations]. The seeing was good.

Thickness of the shadow 0″·28(2). The equatorial region was brighter than any other part of the planet. The north belt was fainter than the south one.

October 28. $6^h\ 30^m$. The ring was distinctly visible. The two luminous spots were quite conspicuous on each ansa.

November 3. $6^h\ 30^m$. The ring and condensations were easily seen. The outer condensations were the brighter. These were so distinct that they looked almost as definite as an ill-

defined satellite would be if projected on the ring. Measures were made at $7^h\ 10^m$.

From the following limb :—

The distant one.	The near one.
7″·88(7) [7″·34]	3″·04(6) [2″·84]

From the preceding limb :—

The distant one.	The near one.
8″·14(5) [7″·59]	2″·68(5) [2″·50]

Distance between the centres of the two following condensations :—

4″·40(4) [4″·10]

Thickness of the trace of ring on the ball :—

0″·71(4) [0″·66]

The position of the centre of the trace was measured, but the seeing was so poor that the result was uncertain.

From North Limb.	From South Limb.
9″·21(2) [8″·59]	8″·82(2) [8″·23]

$7^h\ 50^m$. The inner condensation was a little the brighter in each case. It looked as if it might be double, but the seeing was not good enough to decide. Between the two condensations the ring was almost discontinuous. From the inner condensation to the ball the ring was easily seen to join up to the ball, and was but little less bright than the adjacent condensation. The outer ones seemed to be more brightly condensed, or like very small blurred stars. The inner condensations were about as bright as the faint satellite (Tethys) preceding, and somewhat brighter than the distant ones (condensations).

Assuming that these luminous places were symmetrical with respect to the ball, and taking the means of the measures, we find the distance of the nearest condensations from the limb was

2″·86 [2″·67]

Distance of the outer condensations from limb was

8″·01 [7″·46]

November 5. Very bad sky—thick, with misty clouds.

$10^h\ 17^m$. In moments of steadiness and clearness the distant condensation in the preceding ansa looked like a small, ill-defined satellite. There was a faint satellite (Enceladus) close to it, and it was not possible from their appearance to tell which was the satellite. They were the same size, same form, and same brightness—small hazy spots. The nearer condensation was the brighter, and at times it seemed to be double. The space between it and the ball was nearly as bright as the condensation. The ring between the distant and near condensations was faint and almost discontinuous. The full extent of the ring was seen with difficulty because of the poor sky.

At $10^h\ 22^m$ the following measures were made between the preceding limb and the condensations:—

Distant condensation.	Near condensation.
8″·17(3) [7″·63]	2″·82(2) [2″·63]

At $10^h\ 31^m$ the distant condensation and the faint satellite appeared as one. On the following side the nearer condensation was decidedly brighter than the distant one, say by half a magnitude or more. The inner condensation was certainly double. Perhaps a faint satellite was close to it. The trace of the ring on the ball did not appear black, but the seeing was too poor to be sure.

November 12. $5^h\ 45^m$. The condensations were readily visible. There were two considerable satellites of equal brightness, one near the preceding and the other near the following end of the ring.

$5^h\ 50^m$. The condensations were bright and nearly equal. The inner ones were bright up to the ball. The space between them was almost discontinuous.

$6^h\ 30^m$. Seeing very poor. At this time there was a faint satellite (Mimas) between the bright satellite on the following side and the end of the ring. The outer condensations seemed to be perhaps a little brighter than the inner ones, and possibly a little larger. They were all very conspicuous. The outer one, following, was at least two times as bright as Mimas. The small satellite itself (Mimas) was about the same brightness as the ring between the two condensations. There was a faint satellite (Enceladus) close to the preceding end of the ring, which was nearly as bright as the outer condensation on that side. It was a little north, following the condensation. Occasionally the full extent of the ring was seen up to near Mimas.

$7^h\ 0^m$. The faint satellite (Enceladus) preceding and the outer condensations were together at this time, and made quite a large spot. Distance between the two condensations on the following ansa:—

$$= 5''{\cdot}04(4)\ [4''{\cdot}76]$$

Roughly, the length of the outer condensation was—

$$2''{\cdot}10(4)\ [1''{\cdot}98]$$

It was perhaps $2\frac{1}{2}$ times as long as broad, and diffused rather abruptly on each side. I should say that the outer condensation was $\frac{1}{2}$ as bright as the bright satellite following.

$7^h\ 20^m$. The faint satellite (Mimas) following was going back at this time, and was making a double bright spot with the outer condensation. The faint satellite (Enceladus) preceding was then between the two preceding condensations and very slightly north of the line between them. Could not make out anything on the ball except the trace which was seen only once in a while, though it was conspicuous enough when seen. The inner condensation seemed to go up to the ball without much if any loss of light.

The seeing did not permit any accurate measures from the limbs, but the following results were obtained.

Distance of preceding condensation from preceding limb—

$$7''{\cdot}66(6)\,[7''{\cdot}23]$$

The limb was only a blur, from bad seeing. With the planet unobscured in the field, the condensations were quite conspicuous.

$7^h\ 40^m$. At this time the faint satellite (Mimas) was between the two condensations following, but nearer the distant one.

Distance from following limb to outer condensation—

$$7''{\cdot}72(8)\,[7''{\cdot}29]$$

At the time of these measures the faint satellite (Mimas) following was close preceding the outer condensation.

The outer and the inner condensations seemed to be of the same brightness. It looked as if the preceding condensation on the preceding ansa was not quite as bright as the one on the following ansa, but the presence of the faint satellite (Mimas) near the following one may have been the cause of this. A half moon, with bad seeing, made the sky very bright.

The Condensations.

The following table contains the measures of the distances of these condensations from the preceding and following limbs of Saturn. They are reduced to the mean distance (9·5389) of Saturn from the Sun, and are therefore comparable.

Measures of the Condensations.

	Preceding.		Following.	
	Near.	Distant.	Near.	Distant.
	"	"	"	"
1907 July 2	2·85	7·56	...	...
Nov. 3	2·50	7·59	2·84	7·34
5	...	...	2·64	7·63
12	...	7·23	...	7·29
Mean	2·68	7·46	2·74	7·42

It is evident that the condensations are symmetrical with respect to the centre of Saturn. The individual differences are not large when we take account of the indefinite character of the phenomenon.

I have at various times compared the brightness of these condensations with that of Mimas, Enceladus, and Tethys. The following approximate values of the magnitudes of these satellites reduced to the mean distance of Saturn have been supplied me

by Mr. Parkhurst from his unreduced measures of the magnitudes of the satellites of Saturn :—

Mimas . . .	12·3 magnitude.
Enceladus . .	11·7 ,,
Tethys . . .	10·2 ,,

Taking the positions of the condensations from the measures, and comparing them with the dimensions of the ring system (from my own measures in *M.N.*, lvi. p. 171), it is seen at once (Plate 10, fig. 2) that the outer condensations are located on the outer part of the inner bright ring, in a region that is the brightest of the entire Saturnian system. The inner ones apparently fall on the crape ring. These condensations, therefore, would seem to be on the brightest and the faintest parts of the system. As this light is unquestionably transmitted through the material of the rings, it is evident that the above facts are opposed to each other if we assume that the condensations are caused by the crape ring and the outer part of the inner bright ring, or the thinnest and densest portions of the ring system. It is improbable that the two similar phenomena are produced by exactly opposite conditions. In the crape ring there are so few particles that the sunlight would readily pass through with little or no scattering effect or augmentation by additional reflection. It is known that a narrow portion of the inner bright ring is very much brighter than any other part of the rings or ball. The explanation of this is doubtless due to a denser collection in this zone of the small bodies that form the rings—just as the scarcity of them produces the duskiness of the crape ring for the want of material to reflect light. It is probable, therefore, if they are not too densely crowded to let the sunlight shine through among them, that when seen from the under side they will appear brighter than the other portions of the ring by repeatedly reflecting and scattering the transmitted light. Looking at the diagram, it would seem that the outer condensations are due to the inner bright ring alone. It would appear most probable that the outer bright part of the inner bright ring is responsible for both condensations. On this supposition, the inner condensations are due to the two brighter portions of the inner bright ring seen in perspective. Just why there should be a loss of illumination between the condensations is not quite clear at present, unless it is that the greater depth of the rings in perspective in some way militates against either the transmission or reflection—or both—of the solar rays in that direction.

In the description, I have used the word 'condensation' for these luminous appearances, although I believe it to be misleading. Though these bright places look decidedly broader than the thin trace of the ring on the sky, and appear to be condensations *on* the ring, I think it is simply a matter of contrast or irradiation, and that they are not any broader in reality than the trace of the ring where they occur. I have come to this conclusion from the drawings that I have made, and which are communicated with this

paper, where the ring has simply been darkened between the points of greater brightness without changing its original outline. The result is that, when looked at from a distance, the bright places appear broader than the ring, and look like the 'condensations' as seen in the sky. I think, if the definition were perfect, it would be at once evident that this explanation is the true one. That they are not material condensations is shown by the fact that they entirely disappear when the ring is edge on toward us, at which time they should be most conspicuous if they were masses on the ring.

There is one fact that seems to have been brought out strongly at this disappearance of the rings. The sunlight can sift through them just as it does in the case of the crape ring, only to a very much less degree. The rings were visible when there was no direct sunlight shining upon the surface presented to us—the Sun being on the opposite side to that of the Earth. This phenomenon might be explained by either of two suppositions: that the rings are self-luminous, or that the sunlight sifts through among the particles composing them and thus makes them visible. Inasmuch as the rings have been proved to consist of discrete particles, their temperature cannot be high, and they are therefore not likely to be self-luminous. The second supposition seems to be a simple and sufficient explanation. This is not improbable, for the rings are extremely thin, as no trace of the illuminated edge could be seen at any time while they were supposed to be invisible. Notwithstanding the fact that the sunlight may thus penetrate entirely through even the bright rings, it does not do so with sufficient intensity to illuminate a satellite when in their shadow; for although Japetus was visible at the eclipse of November 1, 1889 (*M.N.*, vol. l. pp. 107–110), through the entire shadow of the crape ring, it was wholly invisible in a 12-in. telescope when it was in the shadow of the bright rings. In fact, though the rings are translucent, they are not transparent.

I have excluded the idea that the illumination is due to reflection of the sunlight from the ball of Saturn, for the further part of the ring was visible—the entire surface—which could not be so by reflection from Saturn.

P.S.—1908 *March* 3.—In looking over my observations of 1907 July 2, I find a note which has a bearing on Professor Aitken's measures of two condensations at the place of the inner ones. The note, which is attached to the sketch of the inner condensation preceding, says: "Possibly here there are two condensations." It will be seen, in my later notes, I also suspect the inner condensation of being double; but the seeing was never so good that I would be sure enough of it to make measures.

Previous Disappearances of the Rings.

I have gathered the following facts from the American Ephemeris and elsewhere about some previous disappearances of the rings:—

In 1848 the Earth passed the plane of the ring in April, going south. On September 3 the Sun passed the plane, going south. On September 13 the Earth passed back north, and on January 19, 1849, the Earth once more went south with the Sun. Therefore between April 12 or 13 and September 3 the Earth and Sun were on opposite sides of the ring. This was again the case between September 13 and January 19.

1861 November 22. The ring disappeared by the Earth passing through its plane to the north—the Sun being some $2\frac{1}{2}^\circ$ south.

In 1862 it reappeared on January 31, the Earth passing through the plane of the ring to the south. On May 17 it disappeared by the Sun passing through the plane to the north. It reappeared on August 12, the Earth passing to the north.

1878. The ring disappeared on February 6, the Sun passing to the south. It reappeared March 1 by the Earth passing to the south.

In 1891 the ring disappeared, September 22, by the Earth passing to the north. It reappeared, October 30, by the Sun passing to the north.

1892, about May 20, the Earth was less than $\frac{1}{2}^\circ$ north of the plane of the rings, but it did not come any nearer, and the ring did not disappear.

1907. The American Ephemeris does not give the times of disappearance and reappearance, but one can deduce them from the "Apparent Elements of Saturn's Ring," p. 515. I have already given Professor Struve values of these quantities for the present apparition of the planet. For completeness of this list I will repeat them here.

1907 April 17. The ring disappeared by the Earth going south.

July 26. It reappeared by the Sun passing south.

October 4. The ring disappeared by the Earth passing north.

1908 January 7. It reappears, the Earth passing south.

The following table, containing the mean times of meridian transit and the declinations of Saturn at the critical times for the phenomena of the disappearance and reappearance of the ring, may be of interest in connection with the present and for comparison in future disappearances of the ring. The last column indicates the cause of the disappearance or reappearance.

Date.	M.T. of Transit. h m	Declination. ° ′	With reference to Plane of Ring.
1848 Apr. 22	21 24	−5 17	⊕ going S.
Sept. 3	12 44	−5 5	☉ ,, S.
,, 12-13*	12 6	−5 22	⊕ ,, N.
1849 Jan. 19	3 36	−5 20	⊕ ,, S.
1861 Nov. 22	19 20	+5 15	⊕ ,, N.
1862 Jan. 31	14 48	+5 15	⊕ ,, S.

* The meridian passage and declination are given for the 12th.

Date.	M.T. of Transit. h m	Declination. ° ′	With reference to Plane of Ring.
1862 May 17	7 30	+7 38	⊙ ,, N.
Aug. 12	2 8	+5 17	⊕ ,, N.
1878 Feb. 6	2 15	−6 14	⊙ ,, S.
Mar. 1	0 54	−5 9	⊕ ,, S.
1891 Sept. 22	23 24	+5 6	⊕ ,, N.
Oct. 30	21 11	+3 25	⊙ ,, N.
1907 April 17	21 54	−4 48	⊕ ,, S.
July 26	15 40	−3 12	⊙ ,, S.
Oct. 4	10 47	−5 6	⊕ ,, N.
1908 Jan. 7	4 30	−5 3	⊕ ,, S.

From an inspection of this table it will be seen that the present disappearance of Saturn's ring has been an extremely favourable one—the most favourable in forty-five years—comparing in this respect with the disappearances of 1848–9.

In 1848–9 and 1861–2 there were two disappearances and two reappearances each of the ring. In 1878 there was only one disappearance, and the corresponding reappearance, both very unfavourably situated. The ring was invisible for about twenty-two days.

In 1891 there was only one disappearance and reappearance. The conditions were likewise very unfavourable. The ring was invisible some thirty-eight days.

At the present apparition of the planet there were two disappearances and two reappearances.

There is an important series of observations of Saturn made with the 15-inch refractor of the Harvard College Observatory by the Bonds at the disappearance of the ring in 1848. These observations form part of volume ii. of the *Harvard College Observatory Annals*.

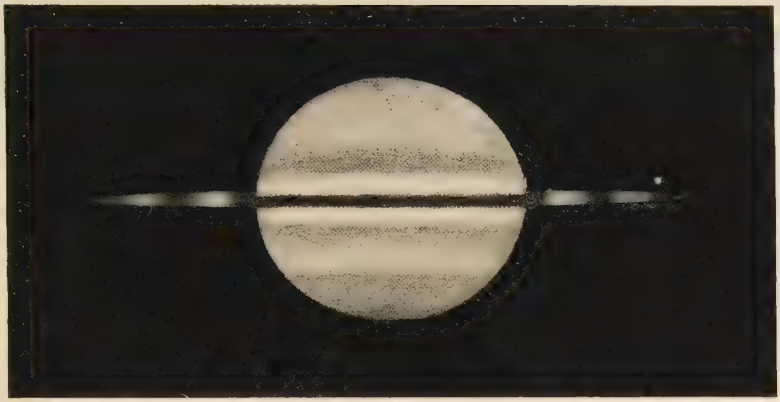
Some of the drawings of Saturn given in the above volume show the ring and condensations essentially as they have appeared here in the past six months.

The conditions of the disappearances and reappearances of the ring in 1848 were almost identical with those we have just witnessed.

In all his observations Bond assumed that what he saw was the sunlit edge of the ring. The explanation of its visibility was therefore an easy one. The observations of the present disappearances have shown, however, that it was not the edge which we saw, but the very oblique unilluminated surface of the ring. The minor axis of the ring about the 1st of July was some 2″.

Bond's explanation of the bright markings, condensations, or knots seen on the ring during its "invisibility" was very ingenious, but it depended upon the edge of the ring being seen through the Cassini division. The extreme thinness of the ring and the largeness of the condensations make this explanation unsatisfactory.

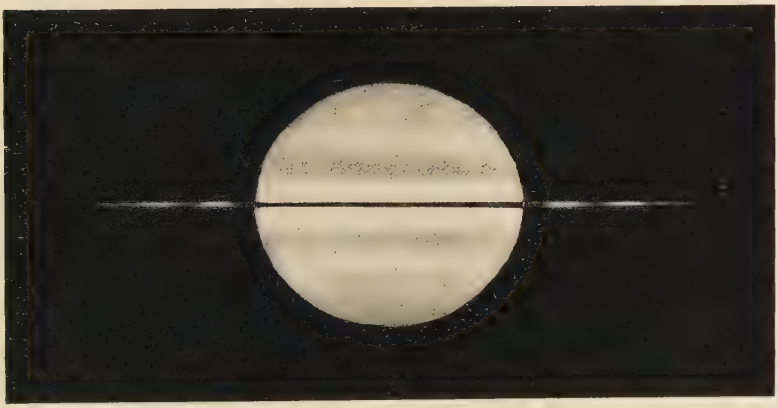
S



N

1907 JULY 2, 15 H. 30 M.

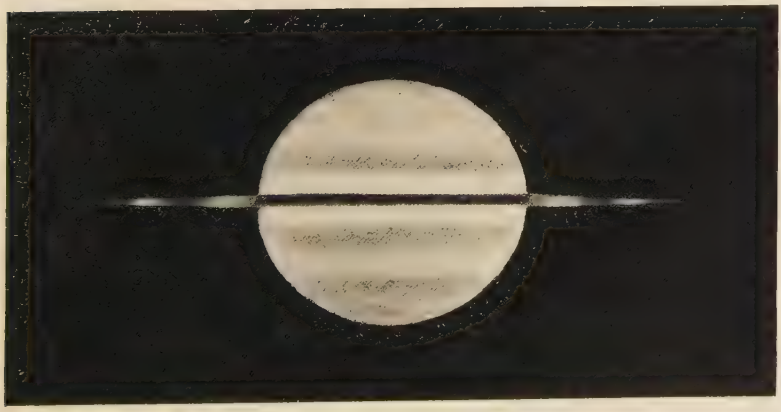
S



N

1907 OCT. 13, 11 H. 30 M.

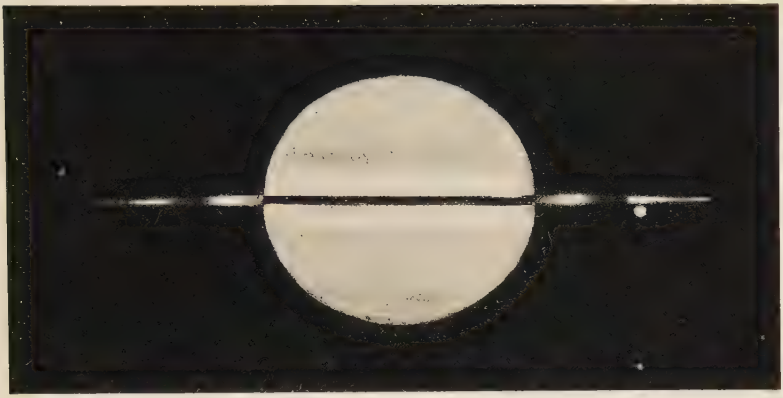
S



N

1907 NOV. 3, 7 H. 50 M.

S



N 1907 Nov. 25 6 H. 0 M.

FIG 1.

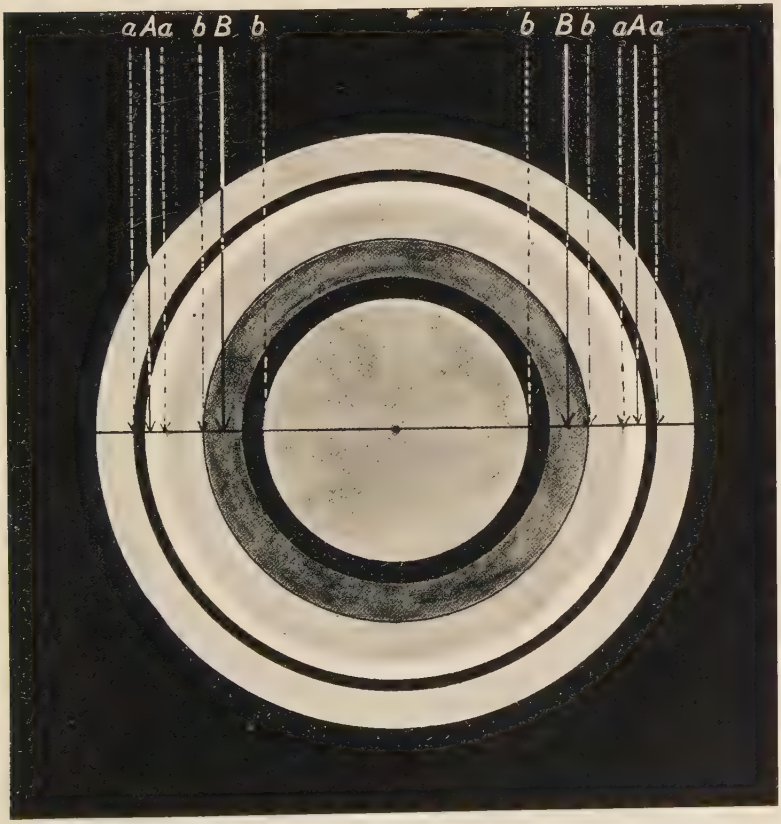


FIG. 2.

Diagram of the ball and ring system of Saturn, showing the projected positions and limits of the condensations.

The lines A, B, B, A show where the centres of the condensations fall. The dotted lines a, a, b, b, b, b, a, a show the limits of the condensations.

In *Monthly Notices* for November 14, 1862, vol. xxiii. pp. 87–88, there are some observations of Saturn at the time of the disappearance of the ring in that year made at Greenwich and Pulkowa. In both these there are distinct references to luminous appendages, which seem roughly to be the same seen at the observations of this year—1907. The descriptions are not quite in satisfactory accord with their appearance as seen to-day. The ring was supposed to have disappeared on May 17, 1862, by the Sun going north, and to have remained invisible until August 12. At Pulkowa the descriptions seem to indicate only two appendages, one on the preceding, the other on the following ansa. They did not seem to have been perfectly symmetrical.

The Pulkowa observations are :—

"1862 May 20. The aspect of the luminous appendages has not varied since last night, with this difference only—that I estimated the length of the preceding side as 0·65 and the following as 0·5 of the planet's diameter.

"1862 May 21. Images good. Estimated extent of luminous appendages :—Preceding side 0·6, following side 0·4, of the planet's diameter; also the intensity of the light appears much more feeble on the following side. The size of these appendages increased in the neighbourhood of the planet, giving them the form of sharp wedges.

"1862 May 22. Images less favourable than yesterday. Extent of luminous appendages on preceding side 0·6 and on following side 0·5 of the planet's diameter.

"1862 June 3. Image very bad, yet the luminous appendages are still distinctly visible. It appears that the length of the following ansa is a little the greater, but this is not certain."

The Greenwich observations are :—

"1862 May 17. The ring beyond the planet at times just visible on the left of the disk, but on the right only a small faint spot could be seen in the plane of the ring, about $\frac{1}{3}$ the diameter of the disk from the planet's limb. I should have suspected this was a satellite but for its elongated shape. Sky very hazy.

"1862 May 19. Saturn very well seen. The ring distinctly visible on the left of the disk, and fairly but not so distinctly visible on the right. Where it crosses the disk, the under edge is much sharper and better defined than the upper edge.

"1862 May 20. Saturn very well seen; the ring distinctly visible, appearing brighter than it did last night. The Greenwich observations were made by Mr. Carpenter."

There are also some observations by Mr. Wray in the same number of the *Monthly Notices*, in which he saw something of the kind in December of 1861 and January of 1862 with a 7-in. refractor.

Yerkes Observatory,
Williams Bay, Wis. :
1907 *November* 25.

Additional Observations of the Disappearances and Reappearances of the Rings of Saturn in 1907–8, *made with the* 40-*inch Refractor of the Yerkes Observatory.* By E. E. Barnard.

1907 Nov. 23—with 12-inch telescope, $6^h\ 15^m$, power 150±. I could see the two condensations on the preceding part of the ring. They were fairly distinct, but faint. Could scarcely see them on the following side.

$7^h\ 30^m$—using a higher power. The ring and condensations were not so well seen. A satellite closing in following, mixed its light with the condensations on that side. With the low power could see the two condensations on the preceding ansa very distinctly. They appeared equal in brightness. The inner one joined up to the planet with but little change of brightness. There was no doubt but that the following ansa and condensations were fainter than the preceding. The satellite interfered, however. The sky was clear and good, but the image was unsteady. The trace on the ball was apparently dark and without irregularities.

Nov. 24. $5^h\ 11^m$. The condensations were easy. There were two satellites, one preceding and one following, that interfered. The condensations were of equal brightness, both preceding and following. The inner one preceding was bright to the ball. The entire ring was visible with or without the occulter. Seeing = 2 (on a scale of 5). Lost in clouds.

$8^h\ 46^m$. Thinning down near Saturn. The following ansa was certainly the brighter. There was a faint satellite (Mimas) following the following end of the ring. The outer condensation near it was twice as bright as the satellite. Seeing = 1. The full extent of the ring was quite conspicuous when best seen.

$9^h\ 8^m$—with the 12-inch telescope. Could see the condensations. Both Mr. Sullivan and I decided that the following side was the brighter. Seeing poor with 12-inch. (Back to 40-inch.) There was no question but that the following ansa was the brighter. The ring between the condensations was faint. The outer condensation following was two times as bright as the faint satellite following (Mimas). In each case the outer and inner condensations were of equal brightness. Seeing very bad.

Nov. 25. $4^h\ 55^m$. The condensations were of equal brightness. The space between them was almost discontinuous.

$5^h\ 25^m$. Distance between the centres of the preceding condensations—

$$4''{\cdot}60\ (6)\ [4''{\cdot}44].$$

Distance between the centres of the following condensations—

$$4''{\cdot}51\ (6)\ [4''{\cdot}35].$$

Seeing very poor. Went to the 12-inch telescope. Could faintly see the rings and condensations—very faint compared with the view in the 40-inch.

Nov. 26. $5^h\ 55^m$. Seeing fair for moments. The condensations

were very bright, but the ring between them was very faint, and all but discontinuous.

$6^h\ 25^m$. The small satellite following was the same brightness as the inner condensation, but very slightly brighter than the outer one. The following ansa was considerably brighter than the preceding. I could see the ring beyond the outer condensation following,—I think, all of it, but it was extremely faint. The condensations were about the thickness of the diameter of the bright satellite following. The inner condensations were a little brighter at their outer ends. Seeing = 3.

$6^h\ 45^m$. The bright satellite was half a magnitude brighter than the outer condensation.

$6^h\ 50^m$. The outer condensation was exactly midway between the satellite and the following limb. The following ansa and condensations were certainly brighter than the preceding. They were all conspicuous with the planet unobscured. When seen best the condensations appeared to become narrower.

Nov. 28. $5^h\ 4^m$. Seeing very poor. The ring and condensations were conspicuous.

$5^h\ 24^m$. There was a small satellite (Tethys) just north of the ring, and about midway between the condensations following. I think the following ansa was the brighter.

$6^h\ 49^m$. There was a small satellite between the two condensations preceding, nearer the outer one. There was a bright satellite close preceding the end of the ring in line with it. There was a similar satellite at the preceding end of the ring a little north. These satellites were half a magnitude brighter than the condensations. I think the inner condensation following was slightly brighter than the outer one.

$6^h\ 59^m$. Seeing very poor. There was a very faint satellite (Enceladus?) then visible at the preceding end of the ring, close south following the brighter satellite. The seeing was very poor. I could not make out any details on the ball.

$8^h\ 54^m$. The seeing better, though the planet was low. The following ansa was, I think, the brighter. The inner and outer condensations were of the same brightness. The inner ones joined up to the ball with almost full brightness. The space between them looked to be a little less than on some previous nights.

Dec. 3. $7^h\ 42^m$. The condensations were quite bright.

Distance from preceding condensation to following limb—

$$27''{\cdot}25\ (8)\ [26{\cdot}63].$$

From preceding limb to outer condensation—

$$7''{\cdot}55\ (8)\ [7{\cdot}38].$$

The inner condensation was bright up to the planet. Could not make much out of it on account of the blurring. I don't think there had been any change in the condensations. They were brighter than the small satellite (Tethys?) preceding.

Dec. 5. $5^h\ 46^m$. The condensations were bright in moments of steadiness. The following ansa was the brighter.

$6^h\ 11^m$. I went over to the 12-inch telescope. The seeing was very poor. I could faintly see the following ansa, but could get only feeble traces of part of the preceding ansa. I could not be certain of seeing the condensations. Used low and high powers. The trace of the ring on the ball, or the shadow, was fairly well seen.

Back to 40-inch. The ansæ were conspicuous without occulter, as were also the condensations, though the seeing was very bad.

$6^h\ 21^m$. In moments of steadiness could see the ring continuous between the two condensations.

Dec. 10. $7^h\ 50^m$. Through clouds, seeing poor. The ring and condensations were easy. A small satellite was 3″–4″ following the following end of the ring. The outer condensation was brighter than this satellite.

Dec. 12. $4^h\ 38^m$. The definition was fair in moments. The ring and condensations were quite easily seen.

$4^h\ 58^m$. Seeing = 3. The ring was easily continuous between the condensations. Could see the ring beyond the outer condensations as a slender thread of light.

Distance from preceding limb to outer condensation,	7″·78 (6) [7″·73]
„ following „ „ „	7·84 (6) [7·79]
Length of outer condensation following,	2·30 (7) [2·28]
Width of space between condensations following,	2·32 (7) [2·30]

The ring and condensations following seemed to be brighter than those preceding. Sometimes the central part of the outer condensation, following, looked like an ill-defined satellite. A sketch showed it to be 2 to 3 times as long as it was broad. The inner condensation following was continuous up to the ball, but there was a brighter place at its outer end. The outer and inner condensations seemed to be of equal brightness. The ring between them was 6 or 8 times as faint as the condensation and looked much thinner. I could not see anything of the shadow of the ball.

$5^h\ 45^m$. The seeing has been 3 all along. The thickness of the outer condensation following was $\frac{1}{3}$ or $\frac{1}{4}$ its length. A sketch shows that close up to the ball the following inner condensation was quite faint for a second or so.

$7^h\ 6^m$. Dione was in conjunction close north with the outer condensation following. A small satellite (Enceladus?) following the following end of the ring was several times fainter than the condensations.

Dec. 25. $4^h\ 48^m$. The condensations, and the ring in general, were easily visible, but they were much thinner than at the last observation. Could see the full length of the ring. It was very narrow and faint at the ends beyond the outer condensation. The ring on both sides could be seen between the two condensations, though faint.

5^h 8^m. The inner condensation was bright nearly up to the ball, but, perhaps by contrast, it was faint near the ball. Seeing = 3. The trace of the ring on the sky was about on a line with the south edge of the shadow on the ball. The seeing was not quite good enough to tell if the shadow of the ring was irregular. It appeared clear and straight in moments of best seeing. A sketch says that the following inner condensation was not continuous close to the ball.

Estimated with the micrometer wire. The thickness of the condensations, or the ring at these points, was about $1\frac{1}{2}$ times the thickness of the wire, while the outer slender end of the ring was perhaps $\frac{1}{2}$ or $\frac{1}{3}$ the thickness of the wire [thickness of the wire $= 0''{\cdot}1$].

5^h 38^m. A small satellite was at this time exactly midway between the condensations. It was about one diameter of itself south of the line of the ring. The ring at the following side was not continuous up to the ball, but there was a faint place from the ball out for, say, $1''$. Question if this was the shadow of the ball on the ring. I could not be certain of this effect at the preceding side.

1908 Jan. 2. 5^h 50^m. The ring was very thin. There was a satellite at each end. The one following was very close to the ring. There was a very faint satellite close north of the ring, half way out, following. Seeing = 3. Without occultation it was almost impossible to see any trace of the ring on the sky. The condensations were feebly seen as slightly brighter parts of the ring. The ring and condensations were slightly brighter than the faint satellite, but were much less bright than the satellite at the following end of the ring. Same at 6^h 30^m.

Jan. 5. 5^h 0^m. The sky good. Seeing = 2. Not dark enough to see the ring.

5^h 10^m. Could see the ring very faintly with the occulter.

5^h 20^m. The sky was not entirely dark, yet could see the ring faintly, but not well.

5^h 40^m. The ring was very faint and quite thin. I think there was a feeble intensification of the ring at the places of the condensations; that is, it was possibly very slightly brighter at those points, but not thicker. The shadow of the ring on the ball was black and strong. The ring was much more difficult than on Jan. 2. I could not see anything of it without the occulter.

Jan. 6. 5^h 0^m. The sky too bright to see the ring. Seeing very poor.

6^h 0^m. A close watch was kept until this time with occulter. The seeing had steadied a little. During the best moments could not see anything of the ring. Seeing at best = 2. There was considerable glow from want of steadiness. Watched at both sides of the planet. It is possible that with better seeing I might have seen it, but it is doubtful. It must have been excessively faint.

Then went to the 12-inch telescope, but the seeing was poor. Tried high and low powers, but could not see anything of the ring.

Jan. 7. $5^h\ 15^m$. Seeing = 2. The ring was quite easily visible by occultation. It was linear, but rather faint on the following side. On the preceding side could see it well. There was a bright satellite at the preceding end of the ring. The ring must have been much brighter than on the 6th, for I could see it when the seeing was worse than it was at times on that date —when I could not see it at all. It looked hazy and ill-defined. There were no traces of condensations. It was a straight bar, its full extent perfectly uniformly illuminated clear to the end. It was of an ashy color, rather thickish and fuzzy.

$5^h\ 30^m$. There was a faint satellite 6″ or 8″ preceding the preceding end of the ring. It was just as bright as the ring. The sky hazed over. Went to the 12-inch telescope with Mr. Fox. He got a glimpse of the planet in a clear place between the clouds, and saw the ring. Thick sky prevented my seeing the ring with the 12-inch. Moonlight made the hazy sky too bright.

Jan. 8. $6^h\ 15^m$—with 12-inch telescope. The planet seen through break in clouds. Could see the ring faintly both following and preceding. Seeing poor—haze and clouds.

From these observations it would appear that the Earth must have passed the plane of the ring sometime between 1908 January, 5 days $6^h\ 0^m$ and January 7 days $5^h\ 15^m$. Perhaps this occurred not far from the time of the observation of January 6.

The observation of January 7 would seem to indicate that the plane had already been passed some hours before, from the distinctness with which the ring could be seen at that time.

The observation of January the 6th, though not under the best conditions, again showed how thin the ring must really be, for it was not visible with the light of the planet occulted.

I think, however, a more effective criterion of its thinness was offered at the various times when the ring was observed with the Sun and Earth on opposite sides of it. At such times, though the very oblique surface of the ring was visible, nothing could be seen of the edge of the ring itself, which should have been seen as a thin rim of light. This sunlit edge of the ring was not visible at any time, though it was always looked for carefully when the seeing was best.

As it may be important for the aid of others in any investigation of the phenomena presented by the ring in 1907, I may be permitted to copy here my previous measures of the Saturnian system; to these I will add the measures of the condensations. They are all reduced to the mean distance of Saturn from the Sun =9·5389. (See *M.N.*, vol. lvi. p. 171.)

Table of Measures of the System of Saturn.

Equatorial diameter of Saturn . .	17″·800	Radius	8″·900
Outer diameter of outer ring . .	40·108	,,	20·054
Inner diameter of outer ring . .	35·046	,,	17·523
Centre of Cassini division . .	34·517	,,	17·258
Outer diameter of inner ring . .	33·988	,,	16·994
Inner diameter of inner ring . .	25·647	,,	12·823
Inner diameter of crape ring . .	20·528	,,	10·264
Width of Cassini division . .	0·529		

Inner condensations from limb . . .	2″·706	{ from centre of planet }	11″·606
Outer edge of inner condensations from limb	...	,,	12·900
Inner edge of outer condensations ,,	...	,,	15·200
Centre of outer condensations . ,,	7·438	,,	16·338
Outer edge of outer condensations ,,	...	,,	17·480

The polar diameter of Saturn was 16″·241.

The measures of December 12 made the length of the outer condensations $=[2''\cdot28]$, and the distance between their nearest edges $=[2''\cdot30]$. From these and the tabulated position of the centre of the outer condensations we have the values of the positions of the ends of the condensations as given in the table. The observations show that the condensations on the preceding and following ansæ were perfectly symmetrical with respect to the centre of Saturn.

If now we compare the full extent of the outer condensations with the Cassini division we have—

Outer edge of outer condensation from centre .	17″·48
Outer edge of the Cassini division . . .	17·52

Likewise we have for the inner condensations—

Outer edge of the inner condensation . . .	12″·90
Outer edge of the crape ring	12·82

These would seem to connect the condensations directly with the Cassini division and with the crape ring.

They are opposed to the supposition that the bright part of the inner bright ring is responsible for the outer condensations, for they show that the outer condensations extended beyond this ring.

I believe it has been shown that the Cassini division must be free of particles, because of the disturbing action of certain of the satellites. If this were not so, and the Cassini division were filled with particles as closely clustered as they are in the crape ring, a satisfactory explanation of the condensations would be that they

were simply due to the sunlight shining through and illuminating the particles in the crape ring for the inner condensations, and a similar effect of the Sun shining through the Cassini division and illuminating the particles in it would produce the outer condensations.

The fact that the inner and outer condensations were essentially of the same intensity would require that the particles should be as closely clustered in the Cassini division as they are in the crape ring.

With the assumption that particles do exist in the Cassini division, the above explanation of the condensations would be satisfactory.

In any attempt to connect the inner condensations with the crape ring, it may be important to state that these condensations have always appeared much brighter than the crape ring has ever appeared to me—even allowing for that ring being ordinarily seen between two bright regions, the inner bright ring and the ball of Saturn.

In conclusion, I am greatly indebted to Professor Frost for the extra time with the 40-inch to observe the phenomena of the disappearances and the reappearances of the ring of Saturn, and for the kind interest he has shown in the work.

Yerkes Observatory,
Williams Bay, Wis.:
1908 *January* 10.

A few Observations of the Planet Saturn and his Rings in the Years 1897–1904. By E. E. Barnard. (Plate 11.)

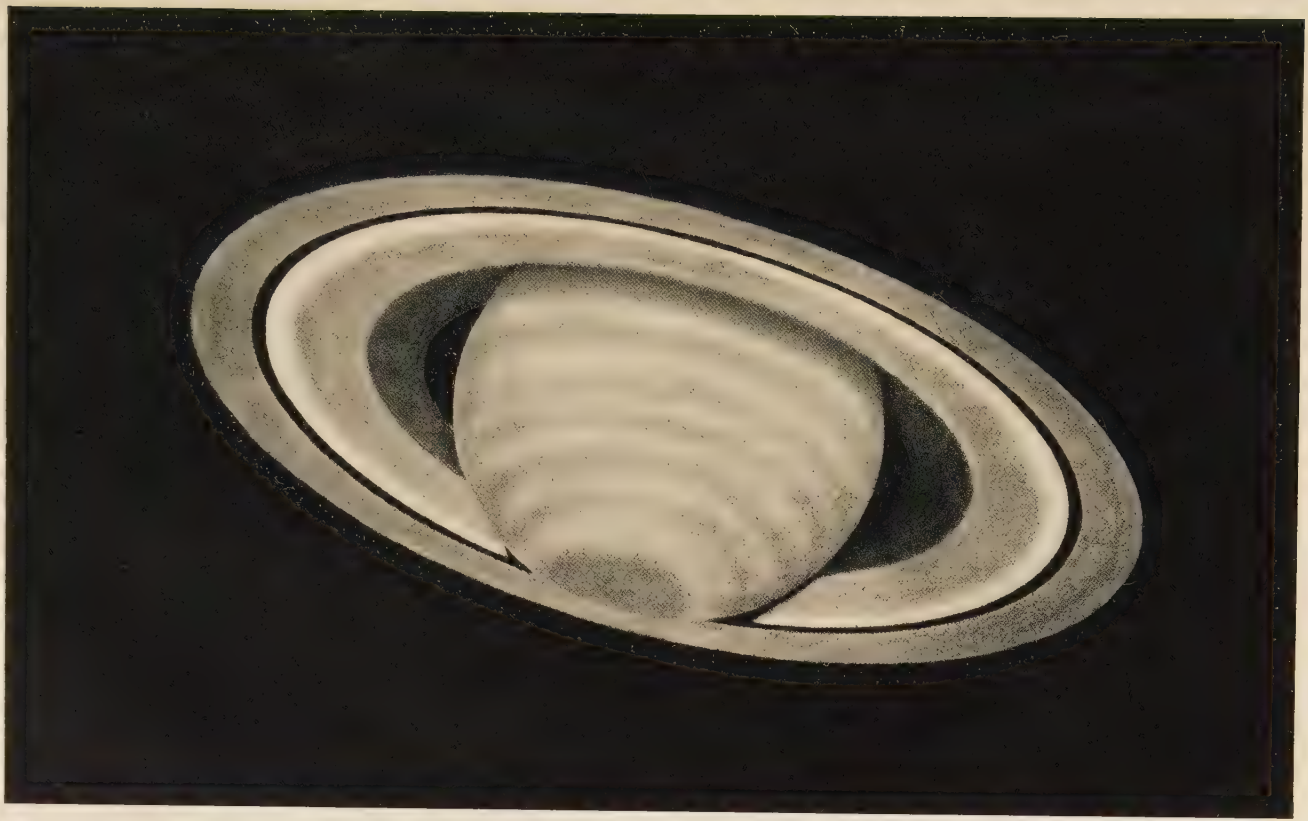
In connection with the papers on the disappearance of the ring of Saturn, I have thought it might be interesting to include in a separate short paper some other notes on the planet.

I have a number of observations of Saturn made here in the past ten years. Some of these are perhaps worthy of record, as they bear on the appearance of the planet with the large telescope when the rings were wide open, and I have collected a few of them for this paper.

1897 May 24. The north polar cap of Saturn was of a bluish dark colour—not well seen. It was bordered by a light belt.

1897 July 1. (With the 12-in. telescope.) The rings were opened so that their outer edges seemed to be exactly coincident with the polar limb of Saturn. It appeared perfectly so on this date, and also on June 29. The shadow of the ball on the rings seemed to be "squarey" where it struck the Cassini division. The polar cap was dark grey and not large. It was bordered by a light space, then a delicate dark narrow belt. There was no light equatorial zone.

I think this angular or "squarey" form of the shadow of the ball on the ring was simply caused by the shadow falling on the



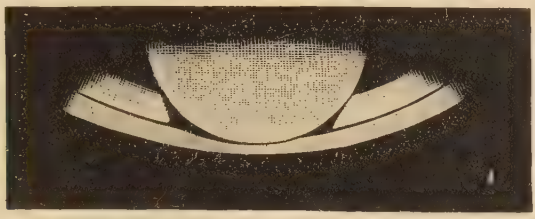
Saturn.

With the 40-inch refractor of the Yerkes Observatory, 1898 July 7, 9 h. 20 m.—E. E. Barnard.

Cassini division and producing thus a kind of "black drop" effect by the junction of the black shadow with the black space.

1898 February 26. The shadow of the ball on the rings was not a uniform curve, but of an angular outline, as shown in the

South.



Form of the shadow of the ball on the Ring of Saturn, 1898 February 26.

sketch. The north pole was dark—not black. The ball was clearly seen through the crape ring.

1898 March 6. The shadow of the ball on the rings preceding seemed "squarey." The north pole was dark.

1898 April 20. There was no definite dark polar cap such as was seen last year, but the polar regions were dark. The planet was beautifully seen through the crape ring. The shadow of the ball on the rings looked "squarey."

1898 July 7. The trace of the crape ring was hazy at both edges, or rather the inner edge of the bright ring was ill defined where it crossed the ball, but it was well defined on the sky between the ring and the ball. The crape ring was easily seen and well defined on the sky, but faint where it approached the ball. The two northern belts were more in contrast, or darker near the limbs of the planet. The inner bright ring was brightest toward the Cassini division, where the brightness was rather narrow. I could not see with certainty any division in the outer ring. There seemed to be a dusky shading where the Encke division is usually shown. The polar cap was darker than the darkest part of the ball.

The definition was superb. I have never seen the planet better, nor have I seen so much detail upon it before. The belts, as shown on the drawing (Plate 11), were seen with certainty. The ball was easily visible through the crape ring.

1899 April 7. $16\frac{1}{2}^{h}$. A heavy diffused belt at the equator. The north pole seemed dark. Seeing poor.

1899 April 25. The north pole was not very dark. The black, well-defined cap was not present. It was a little dusky at the pole.

1901 October 21. $17^{h}\ 50^{m}$. There was no polar cap. There seemed to be a luminous appearance near the north pole, at a point inside the following limb near the shadow; seeing fair, but planet very low.

1904 June 17. There was a heavy, diffused, broad dark belt north of the equator.

1904 July 2. The north belt was very heavy and diffused.

1904 July 4. The north belt was heavy and broad.

1904 July 11. The north polar region was of a light yellow colour.

1904 August 27. The north polar region was light-coloured.

1904 December 5. Light at the equator, with broad dusky region toward the north, but all the north region was lighter.

I have purposely withheld (for another paper) the observations of Saturn in 1903, at the time of the appearance of the white spot on the ball of the planet.

Unless otherwise stated, all the observations in these papers were made with the 40-inch telescope.

Yerkes Observatory,
Williams Bay, Wis.:
1908 *January.*

Photometric Measurements of Saturn, August to December 1907. By J. M. Baldwin, M.A., 1851 Exhibition Scholar (Melbourne).

(*Communicated by Sir David Gill.*)

During the recent opposition of Saturn, at the suggestion of Professor Müller, I undertook a series of photometric measures of that planet. This opposition has been of particular interest, for, owing to the small elevation of the Earth above the plane of the ring, the reduction to "ring invisible" is subject to very slight uncertainty, and so the variation in the brightness of the spheroid itself with phase angle can be obtained.

Seeliger* has theoretically arrived at an expression for the magnitude of Saturn, in which the magnitude depends on two quantities, the first of which, expressing the light received from the ring, shows a marked variation with phase, while the second, expressing the light from the spheroid, is almost independent of phase. Müller,† on the other hand, has arrived at empirical formulæ for the magnitude, in which the variation in magnitude is proportional to the change in phase. In discussing his observations and those of Zöllner, he remarks (*l.c.*, p. 343), "es scheint also, als ob auch bei gänzlich verschwundenem Ring ein Einfluss der Phase sich geltend mache"; and Pannekoek‡ has further emphasised this point, showing that the observed change of brightness with phase when the elevation of the Earth above

* H. Seeliger, *Abhandl. der Bayer. Akad. der Wissensch.*, Bd. 16, 403, 1888.

† G. Müller, *Publ. des Astrophys. Observ. zu Potsdam* 8, 339 u. 341, 1893.

‡ Ant. Pannekoek, *Ast. Nach.* 4006, Bd. 167, 363, 1905.

the plane of the ring was small are greater than accounted for by Seeliger's theory, and so rendering it probable that the spheroid itself shows changes in brightness with the phase. The object of this series of measurements was therefore to determine which of the two formulæ agreed best with observation.

In order to avoid bias, I was careful not to find out the magnitudes of the variation in phase given by the formulæ; and further, the observations were not reduced at all until after the whole series was completed.

The instrument used throughout was the Zöllner photometer referred to in the Potsdam publications as C III, the objective of which is of 21·5 mm. aperture and 137 mm. focal length. (For description see *Publ. Astr. Obs. zu Potsdam*, Bd. viii., p 17, 1891.) In this instrument the image of the planet differs but little from that of a star. Unfortunately there was no suitable comparison star near Saturn; α Aquilæ, distant some 60°, was chosen as the most convenient and was used throughout. Its magnitude has been taken as 0·96 (*l.c.*, p. 235), so that Saturn should be referred to the same system as former planetary observations here have been. For the extinction correction the mean values for Potsdam have been used.

A typical set of observations and the method of reduction is shown in Table I.

Table I.

1907 Sept. 24.	Potsdam Sid. Time.	Quadrant.						Zenith Extinction.			
		I.	II.	III.	IV.	Mean I.	Log $\sin^2$ I.	$\frac{d \log \sin^2 I.}{}$	Dist.	Corr.	ΔM.
	h m	°	°	°	°	°			°		
Saturn	23 12	29·1	24·9	29·4	30·5	28·47	9·3566	+111	57·5	+199	+·08
α Aquilæ	17	31·9	26·0	28·7	28·9	28·87	9·3677	...	61·0	...	...
α Aquilæ	19	29·4	24·1	32·8	26·3	28·15	9·3475	...	61·3	...	...
Saturn	22	28·0	26·9	27·3	30·0	28·05	9·3446	+ 29	57·3	+229	+·06
Saturn	25	30·9	27·0	32.8	27·6	29·57	9·3867	−428	57·3	+324	−·03
α Aquilæ	29	27·1	26·1	30·9	28·0	28·02	9·3439	...	62·7	...	...
α Aquilæ	33	30·4	26·1	27·3	27·2	27·75	9·3360	...	63·3	...	...
Saturn	36	32·2	26·1	29·0	29·0	29·07	9·3732	−372	57·2	+372	·00

In a few cases, however, only eight readings for each were taken, and occasionally α Aquilæ was observed first.

The results of the observations are given in Table II.

TABLE II.

	G.M.T.			Z_S.	Z_α.	ΔM.	Corr.	M_0.	α.	A.	A′.	$M_{M'}$.	O—C.	M_S.	O-
	d	h	m	°	°				°	°	°				
1	Aug. 11	10	55	66·0	46·7	+0·22	−·04	1·14	3·77	−1·82	−0·23	0·96	+·18	0·93	+
2	14	11	4	63·8	48·1	+0·08	·03	1·01	3·52	1·74	0·27	0·95	+·06	0·93	+
3	16	10	30	66·7	46·3	+0·37	·03	1·30	3·34	1·69	0·30	0·95	+·35	0·93	+
4	20	10	45	63·4	48·7	+0·08	·02	1·02	2·97	1·56	0·36	0·94	+·08	0·93	+
5	Sept. 4	10	30	59·9	53·1	+0·12	·01	1·07	1·46	1·05	0·59	0·89	+·18	0·92	+
6	Sept. 10	11	2	57·3	59·9	+0·13	−·01	1·08	0·83	−0·83	−0·68	0·88	+·20	0·92	+
7	11	10	29	58·4	56·1	+0·06	·01	1·01	0·73	0·79	0·69	0·87	+·14	0·92	+
8	12	10	20	58·6	55·6	−0·02	·01	0·93	0·63	0·76	0·71	0·87	+·06	0·92	+
9	13	10	56	57·2	60·8	+0·02	·01	0·97	0·53	0·72	0·72	0·87	+·10	0·92	+
10	18	10	31	57·3	60·1	−0·01	·01	0·94	0·28	0·53	0·80	0·86	+·08	0·92	+
11	Sept. 22	10	7	57·8	59·0	+0·02	−·01	0·97	0·59	−0·39	−0·86	0·89	+·08	0·92	+
12	24	10	21	57·3	62·1	+0·03	·01	0·98	0·80	0·32	0·89	0·90	+·08	0·92	+
13	25	9	52	57·9	58·6	−0·09	·01	0·86	0·90	0·28	0·90	0·90	−·04	0·92	−
14	27	10	5	57·5	61·4	−0·04	·01	0·91	1·11	0·21	0·93	0·92	−·01	0·93	−
15	28	9	2	59·8	53·6	−0·04	·01	0·91	1·21	0·17	0·95	0·92	−·01	0·93	−
16	Sept. 29	9	29	58·3	57·6	−0·10	−·01	0·85	1·32	−0·14	−0·96	0·93	−·08	0·93	−
17	Oct. 2	9	27	58·0	58·8	+0·02	·01	0·97	1·63	0·04	1·01	0·95	+·02	0·93	+
18	3	9	17	58·2	58·1	0·00	·01	0·95	1·74	0·00	1·02	0·95	·00	0·93	+
19	8	9	27	57·7	62·1	+0·13	·02	1·07	2·25	+0·16	1·10	0·97	+·10	0·93	+
20	11	8	51	58·2	58·8	−0·04	·02	0·90	2·55	0·25	1·14	0·98	−·08	0·92	−
21	Oct. 20	8	51	57·9	63·6	+0·16	−·03	1·09	3·39	+0·50	−1·27	1·00	+·09	0·92	+
22	24	8	48	58·1	65·5	+0·01	·03	0·94	3·74	0·59	1·33	1·01	−·07	0·92	+
23	Nov. 3	8	23	58·5	67·4	+0·19	·05	1·10	4·51	0·77	1·48	1·04	+·06	0·92	+
24	27	6	2	58·3	61·0	+0·20	·09	1·07	5·69	0·90	1·84	1·08	−·01	0·91	+
25	Dec. 1	4	57	59·7	54·3	+0·13	·09	1·00	5·78	0·87	1·90	1·09	−·09	0·91	+

1. Hazy near horizon.
2. Only eight observations of each. Wt
3. Seeing good; at times cloudy in parts. Wt. ½.
10. Only eight observations of each. Clouded over suddenly. Wt. ½.
11. Light cloud visible near moon.
14. Slightly foggy. Wt. ½.
16. Cloud rising slowly below α Aquilæ. Wt. ½.
18. Eight observations; seeing below α Aquilæ bad. Wt. ½.
21. Somewhat foggy; cloud rising below α Aquilæ. Wt. ½.
22. Seeing not good, especially below α Aquilæ. Wt. ½.
23. α Aquilæ very unsteady.

In Table II., Z_s and Z_α are the zenith distances of Saturn and α Aquilæ respectively for the mean time of observation; ΔM is the observed difference of magnitude (Saturn—α Aquilæ) corrected for extinction; the next column contains the correction to mean opposition, and M_0 is the magnitude for mean opposition; α is the phase angle; A the elevation of the Earth; A′ that of the Sun above the plane of the ring; M_M and M_s are the magnitudes calculated from the formulæ of Müller and Seeliger, in each case using Müller's values for the constants (*l.c.*, pp. 339, 341, and 348); and O—C are the v lues of $M_0—M_M$ and $M_0—M_s$ respectively.

On examining the column M_0 it is seen that the early observations are irregular; this is probably due to a large extent to want of practice with the instrument, these being the first observations that I had made with it, although I had been using another Zöllner photometer (photometer D) for stellar work for some months. The irregularity may also arise partly from uncertainty in the extinction correction in the first four observations, where the difference in zenith distance is so large (15 to 20 degrees). If the night were not very clear, the difference in extinction would be greater than that given by the Potsdam tables, the values of ΔM would be diminished, and the observed values would agree better with the computed. On account of this irregularity I give the results deduced

(1) from all the observations,
(2) from the observations after opposition (Nos. 10–25),

the conclusions for the latter being, in my opinion, more reliable than from the whole series.

From Table II. it appears—

(1) That if Seeliger's formula were true, the magnitude of Saturn during the observations would be almost constant, becoming very slightly brighter towards the end of the series. The observations, on the other hand, show that the brightness reduced to mean opposition shows a decided decrease after opposition, and so Seeliger's formula cannot hold. If Seeliger's second formula (*l.c.*, p. 489), which assumes a different law for emanation, be used, the results obtained are almost the same, the difference being slightly more marked.

(2) On the other hand, although the earlier observations of the series do not agree well with the results deduced from Müller's formula, yet those after opposition show a very satisfactory agreement. From the column O—C, giving the weight $\frac{1}{2}$ to certain observations as indicated in the table, the mean error is

(*a*) for all observations $\pm 0^{m}{\cdot}115$ mag.
(*b*) for Nos. 10–25 $\pm 0^{m}{\cdot}069$,,

If the reduction to "ring invisible" is applied to the mean opposition magnitude (this reduction is $+0^{m}{\cdot}081$ on August 11,

·000 on October 3, and + ·040 on November 27), and the results substituted in the formula

$$h = h_0 + a\alpha,$$

the values for the constants h_0 and a determined by the method of least squares are

(*a*) for all observations $h_0 = 0{\cdot}967$, $a = {\cdot}0300$, mean error of $h \pm {\cdot}095$
(*b*) Nos. 10–25 $h_0 = 0{\cdot}916$, $a = {\cdot}0313$, mean error of $h \pm {\cdot}064$
while Müller's values are $h_0 = 0{\cdot}877$, $a = {\cdot}0436$.

As before stated, the second set of values for h_0 and a are probably much more reliable than the first set.

It appears then from the observations that the spheroid of Saturn shows a change of brightness with phase, and the change in magnitude can be well represented by the formula

$$h = 0{\cdot}877 + {\cdot}0436\alpha$$

already obtained by Müller from his long series of observations.

Astrophysical Observatory, Potsdam:
1908 Jan. 24.

Reappearance of Saturn's Ring, January 1908.

By R. T. A. Innes.

The following observations of the ghost-ring and of the reappearance of the ring were made here with the 9-inch refractor.

1907 Dec. 25. Ghost-ring seen on both sides, but more distinct on *f.* side. Shadow of ring quite its own breadth N. of equator and very black—no longer brown.

Dec. 28. Ring-like extension very faint, is more distinct on *f.* side—is to S. of shadow.

Dec. 29 and 30. Ghost-ring still visible.

Dec. 31. Just glimpsed (ghost-ring)—4 satellites seen close to Saturn.

1908 Jan. 3. Ghost-ring invisible—Mimas or Enceladus at *p.* elongation.

Jan. 4. No ghost-ring seen.

Jan. 5. No trace of ring-system—one satellite about 4″ and another about 10″ *f.* Saturn—the nearer of these would be half way along the ring had the latter been visible.

Jan. 6. $5^h\ 30^m$ to $6^h\ 5^m$ G.M.T. Ring shadow appreciably narrower—no trace of ring.

Jan. 7. $6^h\ 20^m$ G.M.T. Bad definition through passing thunderclouds. No trace of ring—two satellites, much closer in than Titan, seen closely *f.* Ring shadow narrow.

Jan. 8. $5^h\ 35^m$ G.M.T. (daylight). Ring visible—it is very fine, and not unlike the ghost-ring of a month ago. From my recollection of the disappearance in October 1907, the ring looks as though it had reappeared about six hours earlier. The ring is brighter *f.* the planet—this was verified by using the erecting eye-

piece referred to in a previous communication, when, had the appearance been subjective, there would have been no reversal. Observation was continued until it clouded over at $6^h\ 15^m$ G.M.T. Three satellites *p.* and Titan *f.*

On the same date, at about 6^h G.M.T., Mr. H. E. Wood, M.Sc., made the following note:—"Ring seen distinctly as a sharp well-defined line on either side of the disc. I consider that it was brighter on the *f.* side. I did not see any points of increased brightness in the trace of the ring."

Jan. 9. Ring brighter—*f.* part decidedly thicker than *p.* part.

Jan. 11. *f.* part of ring is the thicker, but with the erecting eyepiece this is not so certain.

Johannesburg:
1908 *January* 13.

Note on the discovery of a Moving Object near Jupiter (1908 CJ).

(*Communicated by P. H. Cowell, F.R.S., for the Astronomer Royal.*)

Whilst examining a photograph of Jupiter's satellites VI and VII, taken on 1908 February 28 with the 30-inch reflector, a faint object was noticed by Mr. Melotte not far from the sixth satellite.

Photographs taken previously were then examined, and the object was identified and traced back as far as January 27. In all there are eight photographs taken on eight nights. The object is slightly brighter than J. VII, and its motion relatively to Jupiter during the month over which the observations extend is $+70^s{\cdot}58$ in R.A. and $+11'\ 39''{\cdot}2$ in Decl. At present the material available is insufficient to show conclusively whether it is a new satellite or a minor planet.

Provisional positions were obtained by referring the image on each plate to two stars chosen from the *Astronomische Gesellschaft* Catalogue.

More accurate places will be determined in the course of the reduction of the photographs of J. VI and VII.

Observations of Moving Object near Jupiter.

Date and G.M.T.	Apparent R.A.	Apparent Dec.	Exposure.	Object—Jupiter. R.A.	Object—Jupiter. Dec.
1908. d h m	h m s	° ′ ″		m s	′ ″
Jan. 27 12 44	8 45 51·78	+18 5 3·2	64 min.	−0 8·32	−43 15·8
Feb. 1 11 52	43 20·49	17 35·7	111 min.	+0 2·01	41 41·7
3 10 27	42 21·99	22 25·2	80 min.	6·47	41 4·7
22 10 57	33 35·48	19 5 48·9	100 min.	48·45	33 59·0
23 8 34	33 14·23	7 36·6	42 min.	50·56	33 36·7
24 12 25	32 47·26	9 51·2	80 min.	53·04	33 9·3
27 10 59	31 42·99	15 17·9	70 min.	1 0·04	31 59·8
28 11 29	31 21·67	17 5·3	80 min.	+1 2·26	−31 36·6

Observations of the Transit of Mercury at Mauritius, 1907 November 14. By T. F. Claxton.

On the afternoon of the transit the weather was cloudy, but the Sun emerged from behind clouds about one minute before the internal contact at ingress, which occurred at $13^d\ 22^h\ 25^m\ 19^s$ G.M.T., whereas the computed time from N.A. data was $13^d\ 22^h\ 25^m\ 47^s$ G.M.T. The telescope used was a 6-inch equatorial by Cooke, with a diagonal eyepiece, power 80.

The Sun's limb was boiling considerably; the definition of the limb was bad, but of the spots at the centre and near the limb good.

Mercury appeared as a clear-cut black disc, perfectly circular, with no white spot or fringe.

No flashing across of cusps was detected.

Almost immediately after internal contact at ingress the Sun became obscured, and was not visible again until about 15 minutes before internal contact at egress. A careful watch was then made for any distortion, white spot, or disc, but none could be detected in the equatorial.

On examining the planet in the 2-inch telescope of the theodolite, however, with an eyepiece of power 40, undoubted distortion was noticed; but on rotating the graduated coloured glass it was found that the distortion rotated with it, indicating that it was an effect of the glass. No white spot or disc was noticed.

Mr. W. P. Ebbels, who continued watching the planet with the equatorial while I was observing with the theodolite, detected no distortion, white spot, or fringe.

The observed times of egress were as follows:—

	W. P. Ebbels with equatorial.	T. F. Claxton with theodolite.	Computed from N.A. data.
	d h m s	d h m s	d h m s
Internal contact .	14 1 46 24 G.M.T.	14 1 46 31 G.M.T.	14 1 46 40 G.M.T.
Bisected by Sun's limb	47 37 ,,	47 39 ,,	
External contact .	48 36 ,,	48 34 ,,	14 1 49 21 ,,

Mr. A. Walter took 11 photographs of the Sun with the photo-heliograph near the times of ingress and egress, copies of five of which are presented to the Society.

The Perturbations of Halley's Comet in the Past. Third Paper. The Period from 1066 *to* 1301. By P. H. Cowell and A. C. D. Crommelin.

We have again to acknowledge the continued kind assistance of Dr. Smart and Messrs. F. R. Cripps and Thos. Wright, who have done a large portion of the mechanical quadratures for this period.

We commence by indicating some small corrections to the values of n at various returns which we gave in the first paper; these arise chiefly from the introduction of the definite integral for Venus and the Earth from the table on p. 386. The corrected values are, 1531 46″·339, 1456 46″·021, 1378 45″·622, 1301 44″·862.

We next give the full table of perturbations for the revolution 1222 to 1301. It will be seen that the effect of the change of n and the action of the smaller planets has an appreciable influence on our former result.

Revolution 1222 *to* 1301.

Planet.	Limits of u.	$\int dn$.	$\int d\varpi$.	$\int d\zeta$.
Venus	0°– 30°	+″·0069	...″	+198″
,,	30–330	–·0013	...	– 50
,,	330–360	–·0070	...	0
Earth	0– 30	+·0045	...	+128
,,	30–330	–·0049	...	+ 31
,,	330–360	+·0089	...	0
Jupiter	0– 90	–·2444	–186	–7040
,,	90–270	–·0175	–167	+7894
,,	270–360	+·5694	–186	– 343
Saturn	0– 90	–·0590	+ 24	–1555
,,	90–270	+·0569	– 37	+1437
,,	270–360	–·2366	+ 16	+ 35
Uranus	0–360	–·0147	– 1	– 34
Neptune	0–360	+·0010	– 6	+ 219
	Sums	+·0622	–543	+ 920

Hence n at $1222 = 44''\cdot862 - ''\cdot062 = 44''\cdot800$.

$$\text{Theoretical period in days} = \frac{1296000 - 920}{44\cdot800} = 28908^{d}\cdot0.$$

Calculated date of perihelion passage 1222 August 30·7 = Julian day 2167635·7. This is 17 days later than our former provisional calculated value, and 9 days later than the day we adopted for the perihelion passage. As the computations for the revolution 1145 – 1222 indicated a still later date for the perihelion passage in 1222, we have reconsidered our interpretation of the observa-

tions of that year and decided to abandon our conjecture that the date of passage of Yew She Te should be mid-August instead of mid-September, though we still think it probable that the comet was seen in August by some of the Chinese observers, since we cannot otherwise explain the statement that it was seen in China for two months ending October 8.

We now put the perihelion passage on 1222 September 10, with a probable error of several days; the conditions were thus practically the same as at the return of 1682, when the comet was circumpolar in Ursa Major in mid-August; we suppose now that the phrase "La Lune joignit la comète" refers not to the date in mid-August when the comet was first seen, but to September 13, when the two bodies were really close together.

A further advantage of the new date is that it puts the comet considerably nearer Antares on October 8 than the former value did; this accords with the Chinese account. We pass now to the preceding revolution; as before, we provisionally took Hind's value of the perihelion passage, viz. 1145 April 19, for the purpose of calculating the perturbations, and deduce the following quantities:—

Revolution 1145 *to* 1222.

Planet.	Limits of u.	$\int dn$.	$\int d\varpi$.	$\int d\zeta$.
	° °	″	″	″
Venus	0– 30	+·0091	...	+ 275
,,	30–330	+·0063	...	+ 157
,,	330–360	−·0021	...	0
Earth	0– 30	−·0109	...	− 290
,,	30–330	−·0006	...	+ 209
,,	330–360	+·0064	...	0
Jupiter	0– 90	+·8690	−181	+24797
,,	90–270	−·0506	−277	− 3501
,,	270–360	−·9932	+256	+ 153
Saturn	0– 90	+·1276	− 89	+ 3661
,,	90–270	−·0411	+ 56	+ 921
,,	270–360	+·0158	− 72	− 38
Uranus	0–360	−·0118	...	− 42
Neptune	0–360	−·0015	...	− 46
	Sums	−·0776	−307	+26256

Now allowing for the fact that the adopted perihelion passage in 1222 is 11 days later than that previously calculated, the adjusted value of u at 1222 = 44″·820; hence that at 1145 is 44″·820 + ″·078 = 44″·898. And calculated period in days $= \frac{1296000 - 26256}{44{\cdot}898} =$

28280·6 days. Now 1222 September 10 = Julian day 2167646. Hence the calculated date of the preceding passage = 2139365·4 = 1145 April 6·4. This is 12·6 days earlier than Hind's date, a quantity sufficiently small to make it certain that Hind's identification of the comet of 1145 with Halley's is correct. Hind's date may be a few days in error, the observations being too vague to fix it with precision. We take the elements as follows (on the old system), ϖ being computed from the perturbations and Ω from our assumed uniform motion,

$$\left.\begin{aligned} T &= 1145 \text{ April } 19. \\ \varpi &= 294^\circ\cdot5 \\ \Omega &= 39\cdot9 \\ i &= 16\cdot5 \\ q &= 0\cdot6 \end{aligned}\right\} \text{Equinox of } 1145.$$

Motion retrograde.

We now pass to the preceding revolution, Hind's date of perihelion passage in 1066 being provisionally adopted for the purpose of computing the perturbations.

Revolution 1066 *to* 1145.

Planet.	Limits of u.	$\int dn$.	$\int d\varpi$.	$\int d\zeta$.
Venus	0°– 30°	+″·0013	...″	+ 39″
,,	30–330	−·0087	...	− 57
,,	330–360	−·0093	...	0
Earth	0– 30	−·0167	...	− 479
,,	30–330	+·0079	...	+ 190
,,	330–360	−·0096	...	0
Jupiter	0– 90	−·0614	+250	−1845
,,	90–270	+·0710	−204	+7655
,,	270–360	+·2486	−401	− 68
Saturn	0– 90	+·0264	− 8	+ 773
,,	90–270	−·0222	−102	− 604
,,	270–360	+·0096	+ 58	− 2
Uranus	0–360	−·0050	...	0
	Sums	+·2319	−407	+5602

Allowing for the fact that the adopted perihelion passage in 1145 is 12 days later than that previously calculated, the adjusted value of n at 1145 is 44″·918. Hence that at 1066 is 44″·918 − ″·232 = 44″·686. And calculated period in days $= \dfrac{1296000 - 5602}{44\cdot686} =$

28877·0 days. Now 1145 April 19 = Julian day 2139378. Hence the calculated date of the preceding passage is 2110501 = 1066 March 28. This is only 4 days earlier than Hind's date, so that his identification of the famous comet that preceded the Norman Conquest with Halley's comet is fully established. His elements, however, require to be modified, and the following elements, in which ϖ is given its calculated value, and Ω its value by our formula, satisfy the observations with sufficient accuracy, considering the vagueness of the descriptions.

$$\left.\begin{array}{rl} T &= 1066 \text{ March } 27. \\ \varpi &= 293^\circ\cdot3 \\ \Omega &= 38\cdot6 \\ i &= 16\cdot5 \\ q &= 0\cdot6 \end{array}\right\} \text{Equinox of 1066.}$$

According to these elements, the comet would be a morning star, in longitude 343°, latitude +9°, near θ Piscium on April 2, would pass to the evening side of the Sun on April 24, when it would be in Gemini and extremely near the Earth; finally, on May 15 it would be in longitude 148°, latitude −10°, and would then be nearly stationary, its place on May 30 being longitude 149°, latitude −8°. These places accord well with the description from Pingré: "Au jour Ki-ouey (Ap. 2) elle était dans la constellation Che (α, β de Pégase). On le voyait le matin; sa longueur était 7°; elle alla fort vîte ver l'est . . . . le jour Sin-se (Ap. 24) on commença à la voir de soir; . . . . dans ce même temps on l'observa dans les Gemeaux . . . . La comète, en Chine cessa de paraître dans la constellation Tchang (entre le cœur de l'Hydre et la Coupe)."

The conditions in 1145 and 1066 were not unlike those predicted for the return of 1910. In each case the comet was discovered as a morning star, then lost in the Sun's rays; on its emergence it was near the Earth and moved with great rapidity, finally becoming stationary in the neighbourhood of Hydra, where it was lost to view.

Erratum,—M.N., vol. lxviii., No. 2, p. 123.

Revolution 1456 *to* 1531. Venus 330°–360° $\int dn$,

for +″·040 *read* −″·009. Sum of column to be altered accordingly.

The Perturbations of Halley's Comet, 1759–1910.

By P. H. Cowell and A. C. D. Crommelin.

The perturbations by Jupiter and Saturn from 1835 to 1910 were investigated in *M.N.*, vol. lxvii., Nos. 6 and 8. The present paper investigates those produced by Venus, the Earth, and Neptune, for the two periods 1759–1835 and 1835–1910.

As the perturbations by Venus and the Earth at the beginning of the period 1759–1835 have only a very trifling influence on the date of the next return, de Pontécoulant's results have been utilised; they are given in *Conn. des Temps*, 1838, p. 118, and *Mémoire sur le calcul des perturbations de la comète de Halley*, 1829, p. 70.

The sum of his values of dn for Venus from values of u 0^g to 20^g (or 0° to 18°) is $+''\cdot00315$ in sexagesimal seconds. As he has only included the direct action of Venus on the comet, we have to add the action of Venus on the Sun, which is given below. The co-ordinates of Venus and the Earth are taken throughout from our table, *M.N.*, vol. lxviii., No. 2, p. 113. $\sqrt{1-e^2}$ is taken as $=0\cdot254$. The Julian days of perihelion in 1759, 1835 are 2363592·57, 2391598·94.

Table of the Action of Venus on the Sun, 1759, *for* $u = 0°$ *to* 18°.

u.	x'.	y'.	$\frac{1}{r'^3}$.	$-\frac{x'}{r'^3}\sin u$.	$-\frac{y'}{r'^3}\sqrt{1-e^2}\cos u$.
0°	+·06	−·71	2·63	·000	+·236*
1	+·01	·71	2·63	−·001	·473
2	−·04	·72	2·64	+·004	·481
3	·09	·71	2·64	·012	·473
4	·14	·71	2·64	·026	·473
5	·19	·69	2·65	·044	·461
6	·24	·68	2·65	·067	·454
7	·30	·66	2·66	·098	·442
8	·35	·63	2·67	·129	·422
9	·40	·60	2·68	·167	·403
10	·45	·55	2·68	·211	·366
11	·50	·50	2·69	·256	·333
12	·55	·44	2·69	·308	·293
13	·60	·37	2·69	·362	·247
14	·63	·29	2·70	·411	·192
15	·66	·19	2·70	·461	·125
16	·68	−·09	2·70	·508	+·058
17	·68	+·02	2·70	·537	−·012
18	−·66	+·14	2·70	+·550	−·092

* Denotes that the quantity has been divided by 2.

$$\int dn = \text{sum of } \left(-\frac{x'}{r'^3}\sin u + \frac{y'}{r'^3}\sqrt{1-e^2}\cos u\right) = -1\cdot678.$$

The reducing factor to reduce $\int dn$ to seconds of arc $= \frac{6\pi}{365\cdot256} a^{\frac{1}{2}} m' du$ (*M.N.*, lxvii., 2, p. 122). The log of this for Venus = 7·2870.

Hence $\int dn$ (0° to 18°, Sun action) = −″·00325.

Hence total $\int dn$ (0° to 18°) = +″·00315 −″·00325 = −″·0001.

The following is a computation of the direct and indirect action of Venus from $u = 19°$ to $u = 30°$.

u	$x'-x$.	$y'-y$.	z'.	$\frac{x'-x}{\rho^3}$.	$\frac{x'}{r'^3}$.	$\frac{y'-y}{\rho^3}$.	$\frac{y'}{r'^3}$.	dn.
19°	− ·22	−1·22	+·26	−·108	−1·660	−·597	+ ·726	+·822
20	− ·05	1·17	·25	−·029	1·480	·684	1·050	·908
21	+ ·15	1·13	·24	+·095	1·236	·713	1·343	·962
22	·39	1·11	·21	·231	·908	·656	1·604	·958
23	·67	1·10	·17	·305	·479	·500	1·809	·845
24	·96	1·15	·12	·284	·053	·341	1·880	·640
25	1·27	1·23	+·05	·230	+ ·422	·222	1·847	·395
26	1·58	1·36	−·02	·174	·865	·150	1·680	+·114
27	1·87	1·54	·09	·131	1·280	·108	1·380	−·185
28	2·13	1·78	·16	·099	1·590	·083	·935	·471
29	2·36	2·06	·21	·076	1·760	·067	+·386	·715
30	+2·51	−2·38	−·25	+·060	+1·760	−·057	−·259	−·447*

* Denotes that the quantity has been divided by 2.

$$dn = \left(\frac{x'-x}{\rho^3} - \frac{x'}{r'^3}\right) \sin u - \left(\frac{y'-y}{\rho^3} - \frac{y'}{r'^3}\right) \sqrt{1-e^2} \cos u.$$

Sum of $dn = +3·826 = +″·0074$ after multiplication by factor whose log is 7·2870. Hence total $\int dn$ (0° to 30°) = +″·0073.

For the direct action of the Earth on the comet from $u = 0°$ to $u = 30°$ in 1759 we have used de Pontécoulant's figures, revising his values between $u = 13^g$ and $u = 18^g$, where we detected an error. Our revised values for dn are $13^g - ‴·01330$, $14^g - ‴·02585$, $15^g - ‴·01581$, $16^g + ‴·06098$, $17^g + ‴·02172$, $18^g + ‴·00996$ (the symbol ‴ denotes centesimal seconds). Further, his value of log m' for Earth should be reduced from 4·4820 to 4·4781. With these corrections $\int dn$ (0° to 30°) = +″·02003 in sexagesimal seconds. To this we have added the action of the Earth on the Sun, the investigation of which is given below.

u.	x'.	y'.	$-\frac{x'}{r'^3}\sin u$.	$-\frac{y'}{r'^3}\sqrt{1-e^2}\cos u$.
0°	−·61	+·74	·000	−·095*
1	·59	·76	+·010	·195
2	·56	·79	·020	·202
3	·53	·81	·028	·207
4	·49	·84	·034	·215
5	·44	·87	·038	·219
6	·39	·89	·041	·224
7	·35	·91	·043	·228
8	·30	·94	·042	·236
9	·25	·95	·039	·237
10	·19	·97	·033	·239
11	·13	·98	·025	·242
12	−·07	·99	+·015	·241
13	·00	1·00	·000	·242
14	+·08	1·00	−·019	·241
15	·16	+·99	·041	·235
16	·24	·98	·063	·231
17	·33	·95	·093	·223
18	·42	·92	·124	·214
19	·51	·87	·160	·202
20	·61	·81	·198	·183
21	·70	·73	·240	·165
22	·77	·64	·274	·143
23	·86	·51	·321	·112
24	·92	·38	·358	·083
25	·56	·22	·385	·048
26	·97	+·06	·407	−·014
27	·96	−·13	·418	+·029
28	·91	·31	·413	·067
29	·83	·47	·393	·102
30	+·71	−·64	−·175*	+·069*

* Denotes that the quantity has been divided by 2.

$$\int dn = \text{sum of } \left(-\frac{x'}{r'^3}\sin u + \frac{y'}{r'^3}\sqrt{1-e^2}\cos u\right) = +1{\cdot}135.$$

Multiplying by $\frac{6\pi}{365{\cdot}256}a^{\frac{1}{2}}m'du$ whose log is 7·3758 we obtain +″·00270. Hence total $\int dn$ (0° to 30°) = +″·02003 + ″·00270 = +″·0227.

We next give the investigation of the perturbations of the comet by Venus at the return of 1835, including both the direct and indirect portions, and dividing them into the two periods $u = 330°$ to $360°$ and $u = 0°$ to $30°$. The column t gives the number of days before or after perihelion passage.

u.	t.	x.	y.	x'.	y'.	z'.	$\frac{x'-x}{\rho^3}$.	$\frac{x'}{r'^3}$.	$\frac{y'-y}{\rho^3}$.	$\frac{y'}{r'^3}$.	dn.
330°	177·90	−1·835	−2·284	+·595	−·330	−·259	+·0793	+1·5440	+·0638	−·8560	+·265*
331	165·62	1·680	2·214	·446	·523	·237	·1048	1·1610	·0834	1·3620	+·191
332	153·95	1·529	2·144	·256	·652	·191	·1404	·6700	·1174	1·7060	−·160
333	142·89	1·383	2·074	+·051	·711	·128	·1841	+ ·1343	·1753	1·8720	·485
334	132·43	1·243	2·002	−·147	·705	−·058	·2236	− ·3894	·2644	1·8670	·754
335	122·55	1·107	1·931	·324	·643	+·014	·2287	·8628	·3761	1·7120	·941
336	113·23	·976	1·858	·467	·542	·079	·1805	1·2500	·4666	1·4500	1·027
337	104·43	·850	1·784	·574	·411	·137	·0991	1·5410	·4930	1·1040	1·014
338	96·16	·729	1·711	·643	·265	·183	+·0276	1·7305	·4645	·7130	·935
339	88·38	·614	1·637	·675	−·114	·218	−·0167	1·8200	·4170	−·3074	·817
340	81·07	·503	1·562	·676	+·034	·242	·0404	1·8230	·3726	+·0917	·677
341	74·21	·398	1·487	·650	·169	·254	·0517	1·7530	·3400	·4557	527
342	67·78	·298	1·412	·605	·291	·256	·0574	1·6290	·3182	·7836	·374
343	61·76	·203	1·336	·543	·398	·251	·0598	1·4600	·3049	1·0700	·224
344	56·11	·113	1·259	·472	·488	·240	·0616	1·2670	·2995	1·3100	−·087
345	50·84	−·029	1·182	·395	·559	·225	·0635	1·0580	·3019	1·4970	+·034
346	45·91	+·050	1·105	·313	·614	·205	·0654	·8364	·3104	1·6410	·140
347	41·29	·124	1·028	·234	·657	·184	·0689	·6240	·3244	1·7520	·227
348	36·97	·192	·950	·155	·686	·161	·0728	·4125	·3431	1·8255	·296
349	32·93	·255	·872	·078	·704	·137	·0788	·2070	·3729	1·8690	·348
350	29·13	·312	·793	−·006	·713	·114	·0866	− ·0159	·4101	1·8880	·381
351	25·56	·364	·715	+·063	·714	·089	·0961	+ ·1665	·4561	1·8870	·399
352	22·20	·411	·636	·127	·710	·066	·1087	·3352	·5156	1·8740	·403
353	19·02	·452	·557	·186	·698	·043	·1256	·4899	·5926	1·8390	·388
354	16·01	·487	·477	·240	·682	·022	·1481	·6308	·6952	1·7925	·357
355	13·13	·518	·398	·290	·663	+·002	·1785	·7612	·8308	1·7405	·312
356	10·36	·543	·318	·339	·641	−·019	·2160	·8886	1·0154	1·6803	·249
357	7·69	·562	·239	·382	·616	·038	·2687	1·0000	1·2763	1·6130	·151
358	5·09	·576	·160	·422	·588	·057	·3427	1·1035	1·6650	1·5370	+·018
359	2·53	·585	− ·080	·459	·557	·075	·4504	1·1990	2·2765	1·4550	−·179
360	0·00	+ ·587	·000	+·495	+ ·524	−·092	−·5842	+1·2910	+3·3275	+1·3670	−·248

* Indicates that the quantity has been divided by 2.

Sum of $dn = -4·294 = -''·00827$ after multiplication by factor whose log is 7·2844.

Perturbations by Venus, 1835, $u = 0°$ to $30°$.

The values of t and x, y are the same (in reverse order, and with the sign of y changed) as those in the last table.

u.	x'.	y'.	z'.	$\frac{x'-x}{\rho^3}$.	$\frac{x'}{r'^3}$.	$\frac{y'-y}{\rho^3}$.	$\frac{y'}{r'^3}$.	dn.
0°	+·495	+·524	−·092	−·5842	+1·2910	+3·3275	+1·3670	−·248*
1	·527	·489	·109	·7450	1·3720	5·2510	1·2740	1·044
2	·558	·450	·125	−·5700	1·4520	9·1800	1·1710	2·098
3	·584	·409	·141	+2·0200	1·5180	15·6000	1·0634	3·650
4	·610	·364	·157	12·1000	1·5860	+8·2900	·9460	−1·126
5	·632	·316	·172	10·5000	1·6400	−7·5250	·8200	+2·878
6	·652	·264	·188	4·6700	1·6920	6·0350	·6850	2·004
7	·668	·208	·202	2·2500	1·7320	3·6300	·5390	1·111
8	·680	·153	·216	1·2800	1·7610	2·3000	·3963	·609
9	·687	·080	·228	·7720	1·7790	1·5200	·2072	·275
10	·688	+·009	·239	·5120	1·7819	1·0700	+·0233	+·052
11	·682	−·067	·249	·3590	1·7664	·7890	−·1740	−·117
12	·666	·145	·256	·2605	1·7249	·6020	·3756	·249
13	·641	·229	·260	·1950	1·6602	·4743	·5931	·359
14	·603	·315	·260	·1500	1·5618	·3840	·8158	·448
15	·554	·399	·254	·1178	1·4380	·3194	1·0350	·517
16	·488	·481	·244	·0938	1·2700	·2717	1·2515	·563
17	·405	·559	·229	·0757	1·0558	·2357	1·4570	·582
18	·307	·625	·205	·0622	·8024	·2094	1·6337	·572
19	·192	·678	·173	·0518	·5036	·1900	1·7780	·527
20	+·065	·709	·133	·0440	+·1711	·1761	1·8660	·445
21	−·074	·714	·085	·0383	−·1956	·1668	1·8870	·323
22	·219	·687	−·030	·0346	·5811	·1626	1·8235	−·159
23	·362	·622	+·030	·0330	·9654	·1626	1·6590	+·041
24	·491	·518	·091	·0339	1·3150	·1662	1·3870	·266
25	·596	·372	·151	·0387	1·6015	·1745	·9995	·505
26	·663	−·191	·202	·0492	1·7860	·1859	−·5146	·729
27	·677	+·018	·240	·0676	1·8260	·1967	+·0485	·915
28	·629	·236	·257	·0938	1·6950	·1989	·6358	1·026
29	·511	·442	·247	·1198	1·3730	·1816	1·1880	1·027
30	−·329	+·605	+·208	+·1298	−·8798	−·1447	+1·6180	+·445*

* Indicates that the quantity has been divided by 2.

Sum of $dn = -1·144 = -''·00220$ after multiplication by factor whose log is 7·2844.

The next two tables contain the perturbations by the Earth in 1835, the values t, x, y being the same as for Venus.

u.	x'.	y'.	z'.	$\frac{x'-x}{\rho^3}$.	$\frac{x'}{r'^3}$.	$\frac{y'-y}{\rho^3}$.	$\frac{y'}{r'^3}$.	dn.
330°	+·442	+·911	−·030	+·038	+·426	+·053	+·877	+·187*
331	·611	·804	·092	·042	·585	·055	·769	·421
332	·748	·673	·147	·048	·713	·059	·642	·442
333	·848	·525	·194	·055	·806	·064	·499	·439
334	·920	·367	·233	·065	·875	·071	·349	·418
335	·960	·208	·263	·078	·915	·080	·198	·381
336	·973	+·053	·285	·094	·929	·092	+·051	·330
337	·964	−·095	·299	·117	·923	·108	−·091	·269
338	·937	·233	·306	·146	·901	·130	·224	·200
339	·894	·357	·308	·189	·864	·160	·345	·123
340	·841	·470	·304	·247	·816	·201	·456	+·039
341	·778	·568	·297	·335	·759	·261	·553	−·057
342	·710	·653	·286	·465	·697	·350	·641	·167
343	·639	·726	·273	·679	·630	·429	·716	·307
344	·565	·787	·259	1·047	·560	·729	·780	·501
345	·492	·839	·244	1·734	·490	1·140	·835	·805
346	·419	·879	·225	3·690	·418	2·259	·877	1·563
347	·349	·912	·208	6·368	·350	3·281	·914	2·390
348	·282	·938	·191	+9·594	·284	+ 1·279	·945	−2·487
349	·217	·957	·173	−4·989	·219	−11·145	·966	+3·526
350	·155	·971	·156	6·871	·157	7·780	·985	2·918
351	·096	·980	·140	4·130	·098	4·083	·998	1·432
352	+·040	·984	·123	2·582	+·041	2·427	1·003	·722
353	−·013	·987	·107	1·766	−·013	1·629	1·007	·370
354	·062	·986	·092	1·285	·063	1·191	1·009	·174
355	·111	·982	·078	·986	·114	·914	1·007	+·053
356	·157	·976	·064	·787	·161	·740	1·003	−·022
357	·199	·968	·050	·649	·205	·622	·997	·072
358	·241	·959	·037	·547	·249	·536	·990	·105
359	·281	·947	·023	·471	·290	·472	·978	·125
360	−·320	−·934	−·010	−·410	−·331	−·422	−·967	−·069*

* Indicates that the quantity has been divided by 2.

Sum of $dn = +3·774 = +''·00891$ after multiplication by factor whose log is 7·3733.

u.	x'.	y'.	z'.	$\frac{x'-x}{\rho^3}$.	$\frac{x'}{r'^3}$.	$\frac{y'-y}{\rho^3}$.	$\frac{y'}{r'^3}$.	dn.
0°	− ·320	− ·934	− ·010	− ·410	− ·331	− ·422	− ·967	− ·069*
1	·360	·919	+ ·004	·364	·374	·385	·953	·144
2	·400	·903	·017	·325	·415	·354	·937	·145
3	·439	·883	·031	·295	·457	·330	·918	·141
4	·478	·861	·045	·269	·498	·311	·897	·132
5	·516	·837	·059	·247	·539	·296	·874	·119
6	·556	·809	·075	·228	·581	·281	·845	·105
7	·595	·778	·090	·214	·623	·273	·815	·086
8	·635	·743	·106	·201	·665	·265	·778	·064
9	·676	·705	·123	·189	·709	·258	·740	·040
10	·716	·661	·140	·180	·751	·255	·694	− ·010
11	·756	·611	·157	·174	·793	·255	·641	+ ·022
12	·795	·553	·176	·167	·836	·255	·582	·058
13	·831	·489	·193	·163	·874	·259	·514	·097
14	·863	418	·212	·159	·908	·265	·440	·138
15	·894	·341	·229	·157	·940	·276	·359	·183
16	·918	·253	·246	·156	·966	·292	·266	·230
17	·935	·157	·262	·154	·981	·313	·165	·277
18	·943	− ·053	·276	·150	·987	·341	− ·055	·328
19	·941	+ ·059	·288	·144	·983	·379	+ ·062	·380
20	·925	·176	·297	·130	·964	·428	·183	·430
21	·892	·300	·300	·101	·926	·488	·311	·484
22	·841	·425	·299	− ·048	·869	·553	·439	·541
23	·771	·550	·293	+ ·039	·793	·601	·565	·597
24	·678	·670	·279	·150	·691	·599	·683	·638
25	·563	·780	·256	·249	·569	·525	·789	·647
26	·425	·875	·225	·291	·426	·401	·877	·605
27	·267	·947	·186	·274	·266	·277	·943	·521
28	− ·089	·991	·138	·227	− ·088	·182	·975	·407
29	+ ·103	1·000	·081	·177	+ ·100	·121	·975	·280
30	+ ·300	+ ·965	+ ·019	+ ·135	+ ·290	− ·083	+ ·934	+ ·081*

* Indicates that the quantity has been divided by 2.

Sum of $dn = +5{\cdot}889 = +''{\cdot}01391$ after multiplication by factor whose log is 7·3733.

In the above investigations we have restricted ourselves to the determination of the perturbation of n, the mean daily motion, as the examination of several revolutions has shown that the effect of Venus and the Earth on the other elements of the comet's orbit is always negligible. $\int d\zeta$ is found with sufficient accuracy by multiplying $\int dn$ from 0° to 30° by period in days − 80.

$\int dn$ from 330° to 360° does not give any sensible contribution to $d\zeta$. We have uniformly adopted 330° to 30° as the interval over which the perturbations by Venus and the Earth should be determined by mechanical quadratures. Beyond these limits we use the definite integral method described *M.N.*, vol. lxvii., No. 6, p. 391, neglecting the quantity denoted by V_4 as insensible.

The following tables have been constructed, enabling the values of $\int dn$, $\int d\zeta$ to be taken out at sight. The argument is the Julian day of the comet passing the positions $u = 30°$, 330°. Multiples of $224^d{\cdot}7023$, $365^d{\cdot}2614$, for Venus and the Earth respectively, must be added to the Julian day to bring it within the limits of the tables. $\int d\zeta$ for $u = 330°$ is always negligible. The tables have been constructed for an average value of a, but they are sufficiently accurate to use without modification for any revolution. The tabular quantities are to be applied algebraically, with the signs of the tables, to those deduced from the mechanical quadratures.

Venus.

Jul. Day. 239	$u=30°$ $\int dn$.	$\int d\zeta$.	$u=330°$ $\int dn$.
1492	+″·0069	+195″	−″·0013
1501	·0061	172	·0029
1510	·0051	144	·0041
1519	·0040	113	·0052
1528	·0028	79	·0062
1537	·0015	42	·0069
1546	+·0003	+ 8	·0073
1555	−·0008	− 23	·0074
1564	·0019	54	·0071
1573	·0025	70	·0065
1582	·0028	79	·0056
1591	·0030	85	·0046
1600	·0027	76	·0033
1609	·0022	62	·0020
1618	·0014	39	−·0008
1627	−·0004	− 11	+·0004
1636	+·0008	+ 23	·0016
1645	·0019	54	·0023
1654	·0032	90	·0028
1663	·0044	124	·0032
1672	·0055	155	·0031
1681	·0064	180	·0026
1690	·0069	195	·0019
1699	·0071	200	+·0009
1708	+·0071	+200	−·0001

Earth.

Jul. Day. 239	$u=30°$ $\int dn$.	$\int d\zeta$.	$u=330°$ $\int dn$.
1494	+″·0002	+ 6″	+″·0008
1509	·0016	45	·0018
1524	·0029	82	·0025
1539	·0043	121	·0029
1554	·0054	152	·0028
1569	·0064	180	·0026
1584	·0073	206	·0019
1599	·0078	220	+·0008
1614	·0079	223	−·0003
1629	·0076	214	·0018
1644	·0069	195	·0033
1659	·0060	169	·0046
1674	·0049	138	·0059
1689	·0036	102	·0070
1704	·0022	62	·0076
1719	+·0008	+ 23	·0080
1734	−·0003	− 8	·0079
1749	·0012	34	·0074
1764	·0022	62	·0068
1779	·0026	73	·0056
1794	·0027	76	·0045
1809	·0026	73	·0034
1824	·0022	62	·0020
1839	·0012	34	−·0006
1854	−·0001	− 3	+·0005

Hence we obtain $\int dn$ ♀ 1759, $30^\circ = +''\cdot0004$

⊕ 1759, $30 = +\cdot0035$
♀ 1835, $330 = +\cdot0025$
$30 = -\cdot0002$
⊕ 1835, $330 = -\cdot0051$
$30 = -\cdot0025$

These have to be added to the perturbations already obtained by mechanical quadratures.

We next investigate the perturbations by Neptune for the two revolutions 1759–1835, 1835–1910. The co-ordinates of Neptune are taken from the table, *M.N.*, lxviii., No. 2, p. 120.

Revolution 1759–1835.

u.	Interval in years. *t*.	*x*.	*y*.	*r*.	*x'*.	*y'*.	*z'*.	$\frac{x'-x}{\rho^3}$.	$\frac{x'}{r'^3}$.	$\frac{y'-y}{\rho^3}$.	$\frac{y'}{r'^3}$.
0°	0·0	+ ·6	0·0	·6	−28·0	+ 5·7	+9·1	− ·00100	− ·00103	+ ·00020	+ ·00021
18	0·2	− ·3	+1·4	1·4	28·0	5·8	9·1	·00108	·00103	·00017	·00021
36	0·7	2·9	2·7	3·9	27·9	6·4	9·2	·00128	·00103	·00019	·00024
54	2·0	6·9	3·7	7·8	27·5	7·8	9·3	·00170	·00101	·00034	·00029
72	4·1	11·9	4·4	12·7	26·7	10·1	9·4	·00237	·00099	·00091	·00037
90	7·4	17·5	4·6	18·1	25·1	13·5	9·5	− ·00222	·00093	·00259	·00050
108	11·8	23·1	4·4	23·5	22·4	17·7	9·2	+ ·00017	·00083	·00313	·00065
126	17·3	28·1	3·7	28·4	18·1	22·4	8·6	·00083	·00067	·00156	·00083
144	23·7	32·1	2·7	32·3	12·2	26·4	7·5	·00062	·00045	·00073	·00097
162	30·9	34·7	+1·4	34·8	− 4·6	29·2	5·7	·00043	− ·00017	·00039	·00108
180	38·3	35·6	0·0	35·6	+ 3·4	29·6	3·3	·00033	+ ·00013	·00025	·00109
198	45·8	34·7	−1·4	34·8	11·5	27·8	+ ·7	·00028	·00042	·00018	·00103
216	53·0	32·1	2·7	32·3	18·2	23·9	−1·9	·00027	·00067	·00014	·00088
234	59·4	28·1	3·7	28·4	23·0	19·0	4·0	·00029	·00085	·00013	·00070
252	64·9	23·1	4·4	23·5	26·2	13·6	5·8	·00034	·00097	·00012	·00050
270	69·3	17·5	4·6	18·1	27·9	8·9	6·9	·00041	·00103	·00012	·00033
288	72·6	11·9	4·4	12·7	28·6	5·4	7·6	·00053	·00106	·00013	·00020
306	74·7	6·9	3·7	7·8	28·8	3·0	8·1	·00069	·00106	·00013	·00011
324	76·0	2·9	2·7	3·9	28·8	1·5	8·3	·00088	·00106	·00012	·00006
342	76·5	− ·3	−1·4	1·4	28·8	0·9	8·4	·00104	·00106	·00008	·00003
360	76·7	+ ·6	0·0	·6	+28·8	+ 0·7	−8·4	+ ·00111	+ ·00106	+ ·00003	+ ·00003

u.	dn.	$dn\times(2\pi-nt)$.	$(xY-yX)\sin u$.	rX.	rXx.	rYy.
0°	·00000	·00000	·00000	+·00001*	·000	·000
18	−·00001	−·00006	+·00002	−·00007	·000	·000
36	·00014	·00087	·00048	·00097	+·003	−·001
54	·00057	·00349	+·00179	·00538	·037	+·001
72	·00135	·00803	−·00030	·01753	·209	·030
90	−·00129	−·00733	·03064	−·02335	+·408	·174
108	+·00114	+·00606	·05876	+·02350	−·543	·257
126	·00132	·00643	−·02107	·04260	1·200	+·077
144	·00058	·00252	+·00283	·03456	1·110	−·021
162	+·00002	+·00008	+·00714	·02088	·725	−·034
180	−·00021	−·00066	·00000	+·00712	−·253	·000
198	−·00017	−·00004	−·00903	−·00487	+·169	+·041
216	+·00009	+·00018	·01335	·01292	·415	·065
234	·00036	·00050	·01126	·01590	·447	·060
252	·00057	·00055	·00571	·01481	·342	·039
270	·00062	·00037	−·00082	·01122	·196	·017
288	·00051	·00017	+·00144	·00673	·080	+·004
306	·00030	·00005	·00122	·00289	·020	−·001
324	+·00010	+·00001	·00039	·00070	+·002	·000
342	·00000	·00000	+·00002	−·00003	·000	·000
360	·00000	·00000	·00000	+·00001*	·000	·000
Sum	+·00187	−·00356	−·13561	+·01134	−1·503	+·708

* Indicates that the quantity has been divided by 2.

X denotes $\frac{x'-x}{\rho^3}-\frac{x'}{r'^3}$. Y denotes $\frac{y'-y}{\rho^3}-\frac{y'}{r'^3}$.

$dn = \mathrm{X}\sin u - \sqrt{1-e^2}\,\mathrm{Y}\cos u$. $\sqrt{1-e^2}$ is taken as 0·254.

The factor to convert $\int dn$ into seconds of arc $= \frac{6\pi}{365\cdot256}\,a^3m'du$.

$\log\frac{6\pi}{365\cdot256} = 8\cdot7127$, $\log a = 1\cdot2564$ for revolution 1759–1835, $= 1\cdot2480$ for revolution 1835–1910, $\log m' = 5\cdot7122$, $du = 18°$ in seconds of arc, $\log du = 4\cdot8116$, $\log\frac{1}{e} = 0\cdot0145$.

Hence $\int dn = +''\cdot00137$.

The factor to convert $\int dn \times (2\pi - nt)$ into seconds of arc $= 3a^2 m' du$, giving $-11''\cdot6$ for the numerical value of the integral.

$$\int d\varpi = \frac{m'a}{e}\int (x\mathrm{Y} - y\mathrm{X}) \sin u \, du - \frac{m'a}{e}\sqrt{1-e^2}\int r\mathrm{X} du$$
$$= -8''\cdot4$$

$$\int d\zeta = -\sqrt{1-e^2}\int d\varpi - 2m'\int r(x\mathrm{X} + y\mathrm{Y})du + \int dn \times (2\pi - nt)$$
$$= +2''\cdot1 + 5''\cdot3 - 11''\cdot6 = -4''\cdot2.$$

Neptune, revolution 1835–1910.

u.	Interval in years. *t*.	*x*.	*y*.	*r*.	*x'*.	*y'*.	*z'*.	$\frac{x'-x}{\rho^3}$.	$\frac{x'}{r'^3}$.	$\frac{y'-y}{\rho^3}$.	$\frac{y'}{r'^3}$.
0°	0·0	+ ·6	·0	·6	+28·8	+ 0·7	−8·4	+ ·00111	+ ·00106	+ ·00003	+ ·00003
18	0·2	− ·3	+1·4	1·4	28·8	+ 0·5	8·4	·00105	·00106	− ·00003	+ ·00002
36	0·9	2·8	2·7	3·9	28·8	− 0·3	8·5	·00089	·00106	·00008	− ·00001
54	2·0	6·7	3·6	7·6	28·7	1·5	8·7	·00071	·00106	·00010	·00006
72	4·0	11·6	4·3	12·4	28·4	3·8	9·0	·00055	·00105	·00011	·00014
90	7·1	17·1	4·5	17·7	27·7	7·2	9·2	·00043	·00102	·00011	·00027
108	11·4	22·6	4·3	23·0	25·9	12·0	9·5	·00034	·00096	·00012	·00044
126	16·8	27·5	3·6	27·8	22·8	17·2	9·3	·00030	·00084	·00012	·00063
144	23·1	31·4	2·7	31·5	18·0	22·5	8·6	·00028	·00066	·00014	·00083
162	30·0	33·9	+1·4	34·0	11·5	26·8	7·3	·00029	·00042	·00018	·00099
180	37·2	34·8	·0	34·8	+ 4·0	29·3	5·5	·00033	+ ·00015	·00025	·00108
198	44·4	33·9	−1·4	34·0	− 4·1	29·6	3·1	·00043	− ·00015	·00040	·00109
216	51·3	31·4	2·7	31·5	11·5	27·8	−0·7	·00061	·00042	·00076	·00103
234	57·6	27·5	3·6	27·8	17·1	24·6	+1·4	·00080	·00063	·00162	·00091
252	63·0	22·6	4·3	23·0	21·5	20·7	3·3	+ ·00023	·00079	·00348	·00076
270	67·3	17·1	4·5	17·7	24·4	16·9	4·7	− ·00210	·00090	·00357	·00062
288	70·4	11·6	4·3	12·4	26·0	14·0	5·7	·00236	·00096	·00159	·00052
306	72·4	6·7	3·6	7·6	27·0	11·8	6·2	·00173	·00100	·00070	·00044
324	73·5	2·8	2·7	3·9	27·4	10·6	6·5	·00130	·00101	·00042	·00039
342	74·2	− ·3	−1·4	1·4	27·6	9·9	6·7	·00108	·00102	·00034	·00037
360	74·4	+ ·6	·0	·6	−27·6	− 9·8	+6·7	− ·00098	− ·00102	− ·00034	− ·00036

u.	dn.	$dn\times(2\pi-nt)$.	$\frac{(xY-yX)}{\sin u}$.	rX.	rXx.	rYy.	$\frac{(xY-yX)}{\cos u}$	rY.
0°	·00000	·00000	·00000	+·00001*	·000	·000	·00000	·00000
18	+·00001	+·00006	·00000	−·00001	·000	·000	+·00003	− 7
36	−·00009	−·00056	+·00038	·00066	+ ·002	−·001	·00052	27
54	·00027	·00165	·00123	·00266	·018	−·001	·00089	− 31
72	·00048	·00286	+·00172	·00620	·072	+·002	+·00056	+ 37
90	·00059	·00335	−·00009	·01044	·179	·013	·00000	283
108	·00056	·00298	·00434	·01426	·322	·032	+·00141	735
126	·00036	·00175	·00975	·01501	·413	·051	·00707	1418
144	−·00008	−·00035	·01215	·01197	·377	·059	·01677	2176
162	+·00016	+·00060	−·00845	−·00442	+ ·150	+·039	·02603	2754
180	+·00021	+·00066	·00000	+·00626	− ·218	·000	·02889	2889
198	−·00001	−·00003	+·00701	·01972	·668	−·033	·02159	2347
216	·00055	·00107	+·00335	·03244	1·020	−·023	+·00462	+ 851
234	·00127	·00179	−·01991	·03975	1·100	+·071	−·01443	− 1975
252	−·00118	−·00113	·06266	+·02346	− ·530	·269	−·02034	6257
270	+·00120	+·00072	·04504	−·02124	+ ·363	·235	·00000	5217
288	·00141	·00047	−·00603	·01736	·201	·057	+·00196	1327
306	·00063	·00010	+·00072	·00555	·038	·007	−·00052	198
324	·00018	+·00001	·00041	·00113	+ ·003	+·000	−·00057	− 11
342	+·00001	·00000	+·00002	−·00008	·000	·000	−·00006	+ 4
360	·00000	·00000	·00000	+·00001*	·000	·000	+·00001	·00000
Sum	−·00163	−·01490	−·15358	+·01069	−1·398	+·776	+·07443	−·01556

$\int dn = -''\cdot00118$, $\int(2\pi - nt)dn = -46''\cdot8$.

* Indicates that the quantity has been divided by 2.

By the formulæ given above $\int d\varpi = -9''\cdot3$

$$\int d\zeta = -46''\cdot8 + 2''\cdot4 + 4''\cdot1 = -40''\cdot3$$

Again, $$\int de = \frac{m'a\sqrt{1-e^2}}{206265}\left[\int(xY - yX)\cos u\, du + \int rY\, du\right]$$
$$= +\cdot0000043.$$

We have now completed the investigation of the perturbations of the mean motion and perihelion by all the planets whose effect is sensible, with the exception of Uranus. We have decided that it would be waste of labour to repeat de Pontécoulant's work on this planet, seeing that (1) the whole effect on the time of the next return is only 2 days, which is unlikely to be more than 1 day in error; (2) owing to the fact that the period of Uranus is not very different from that of the comet, the perturbations by this planet

change in a fairly uniform progression from one revolution to another. We find that this test is satisfied by de Pontécoulant's Uranus perturbations for the three revolutions 1682–1759, 1759–1835, 1835–1910.

We had originally expected that we should only find it necessary to investigate the revolution 1835–1910, with the single exception of the perturbations by Neptune in the preceding revolution. However, on examination we found large discordances in the different determinations of n in 1835. In particular, the values of de Pontécoulant and Rosenberger differ by $0''{\cdot}05$, implying a difference of an entire month in the date of the next perihelion passage.

In view of this large discordance it seems necessary to re-investigate the Jupiter and Saturn perturbations for the revolution 1759–1835 with the same rigour as the following revolution. We have as yet only made an approximate investigation of these, using the same abbreviations as we employed in the ancient revolutions. The result is decidedly in favour of the Rosenberger result as the more accurate; in fact, it is on the opposite side of this from the de Pontécoulant result. We suspect that the cause of the latter's error is the manner in which he has rectified the orbit in the fourth quadrant; he has made the eccentric anomaly proceed uninterruptedly in the rectified orbit, which implies a want of continuity in the time.

For example at $u = 300^g$ the time shifts abruptly from 25376^d to 25298^d

,, $u = 325^g$,, ,, ,, 26719^d ,, 26747^d

,, $u = 350^g$,, ,, ,, 27563^d ,, 27545^d

,, $u = 375^g$,, ,, ,, 27896^d ,, 27895^d

In other words, portions of the Jupiter and Saturn perturbations are either omitted or reckoned twice; this would have no sensible effect on the date of perihelion in 1835, but it would have an appreciable effect on the mean motion at that epoch, and consequently on the date of perihelion in 1910.

We regard Rosenberger's mean motion in 1835 as presumably nearer the truth, but we do not consider that it can be safely taken as definitive, since he only carried his computations up to 296° of eccentric anomaly, adopting Damoiseau's results for the rest of the revolution; the shift can scarcely fail to have caused some breach of continuity, in addition to which Damoiseau's date of perihelion passage in 1835 was $11\frac{1}{2}$ days too early, which implies appreciable errors in the Jupiter perturbations shortly before perihelion.

Rosenberger contemplated completing his computations as soon as the date of perihelion was known by observation, but we cannot ascertain that he ever carried out his intention. Should anyone know of later researches published by him, we should be very grateful to be supplied with the reference.

We now collect our results in tabular form, with the premise that the Jupiter and Saturn figures for the first revolution are not yet definitive.

Revolution 1759–1835.

Planet.	Limits of u.			dn.	$d\zeta$.
♀	0°– 30°	(with def. integral)		+″·0077	+ 215″
	330–360	,,	,,	−·0058	0
⊕	0– 30	,,	,,	+·0262	+ 731
	330–360	,,	,,	+·0038	0
♃	0– 90			+·0091	+ 242
	90–270	,,	,,	−·2588	+ 790
	270–360			+·6870	− 108
♄	0– 90			−·0072	− 176
	90–270	,,	,,	+·0381	+2086
	270–360			−·1150	+ 23
♅	0–360			+·0094	+ 229
♆	0–360			+·0014	− 4
			Sums	+·3959	+4028

Hence we have the equation $\frac{1296000'' - 4028''}{n \text{ at } 1759} = 28006{\cdot}37$, the observed period in days. Hence n 1759 = 46″·1314

add ·3959

n 1835 = 46·5273

The other determinations of n in 1835 are Rosenberger 46″·5088, de Pontécoulant (latest value) 46″·4586, Damoiseau 46″·4994, Lehmann (evidently affected by some error) 46″·9916.

Revolution 1835–1910.

Planet.	Limits of u.			dn.	$d\varpi$.	$d\zeta$.	de.
♀	0° 30°	(with def. int.)		−″·0024	0″	− 65″	0
⊕	0– 30	,,	,,	+·0114	0	+ 309	0
♃	0–360	,,	,,	+·1465	+222	+31986	−·000097
♄	0–360	,,	,,	+·0084	− 52	− 21	−·000010
♅	0–360			+·0105	− 18	+ 107	+·0000041
♆	0–360			−·0012	− 9	− 40	+·0000043
			Sums	+·1732	+143	+32276	−·000099

Hence $\frac{1296000'' - 32276''}{n \text{ at } 1835} =$ period in days.

Using our provisional value of n 46″·5273
this gives 1910 March 29ᵈ·3 for next perihelion passage.

Using Rosenberger's value of n 46″·5088
this gives 1910 April 8ᵈ·8 for next perihelion passage.

Using de Pontécoulant's value of n 46″·4586
this gives 1910 May 7ᵈ·8 for next perihelion passage.

We are inclined to take 1910 Apr. 8·0 as the most probable value, pending the accurate redetermination of the Jupiter and Saturn perturbations from 1759 to 1835.

We have not yet examined the perturbations of the orbit plane, and so we provisionally adopt de Pontécoulant's values of the node and inclination in 1910 for the purpose of computing a search ephemeris.

$$\left.\begin{aligned} \varpi &= 111^\circ\ 32' \\ \Omega &= 57\ 11 \\ i &= 162\ 13 \end{aligned}\right\} 1910{\cdot}0$$

$$T = 1910\ \text{Apr.}\ 8{\cdot}0$$
$$e = 0{\cdot}96729$$
$$n = 46''{\cdot}69$$
$$\log a = 1{\cdot}2539$$
$$\text{Instantaneous value of period} = 76^{y}{\cdot}00$$
$$q = 0{\cdot}5869$$

These give rise to the equations for the equator

$$x = r[9{\cdot}9852] \sin (v + 145^\circ\ 38')$$
$$y = r[9{\cdot}9884] \sin (v + 239\ 11)$$
$$z = r[9{\cdot}5350] \sin (v + 188\ 55)$$

Search Ephemeris for Halley's Comet.

Date.	Interval.	R.A.	N. Dec.	log r.	log Δ.	u.
	d	h m s	° ′			°
1908 Oct. 1·7	54·3	6 46 40	12 23	0·8168	0·8154	311
Nov. 25·0	50·8	6 25 52	11 31	0·7858	0·7217	313
1909 Jan. 14·8	46·8	5 38 52	11 49	0·7537	0·6817	315
Mar. 2·6	43·4	5 7 48	13 10	0·7202	0·7047	317
Apr. 15·0	40·0	5 10 0	14 52	0·6854	0·7317	319
May 25·0	36·7	5 21 4	16 15	0·6489	0·7329	321
June 30·7	33·5	5 41 48	17 6	0·6110	0·7045	323
Aug. 3·2	30·5	6 1 20	17 29	0·5714	0·6472	325
Sept. 2·7	27·8	6 14 52	17 31	0·5300	0·5629	327
Sept. 30·5	25·0	6 16 36	17 20	0·4868	0·4553	329
Oct. 25·5	22·5	5 58 24	17 21	0·4416	0·3235	331
Nov. 17·0	20·2	5 8 8	17 4	0·3944	0·1885	333
Dec. 7·2	17·9	3 42 28	15 16	0·3452	0·0993	335
Dec. 25·1		2 16 4	11 46	0·2940	0·0987	337

To these may be added the following:—

	h m s	° ′
1908 Jan. 3	6 28 10	10 1
Feb. 2	6 11 19	10 35
Mar. 3	6 0 1	11 23

These were computed for comparison with Dr. Wolf's object BN; as they show a distinctly different rate of acceleration, the identity of the object with Halley's comet seems improbable.

From the beginning of 1910, Dr. Smart has kindly computed an ephemeris at 4-day intervals. In view of the considerable uncertainty in the date of perihelion, some abbreviations have been introduced, the computed heliocentric co-ordinates of the comet in 1835 being utilised.

It will be seen that the comet is an evening star till March 17, and after May 11, being a morning star in the interim. The near approach to the Earth on May 12 and the extremely rapid geocentric motion at that time are noteworthy. It is also evident that at the time of greatest brightness, the conditions of observation are much more favourable in the southern hemisphere than in the northern. It may be worth while adding that the comet will probably be near the Sun at the time of the total solar eclipse of 1910 May 8.

Greenwich Noon.	Right Ascension.	Declination.	Δ	Brightness.	Sun's Place.	
					R.A.	Dec.
1910.	h m s	° ′			h m	°
Jan. 2	1 41 44	+9 30	1·33	1·00	18 49	−23
6	1 28 28	+8 54	1·37	1·00	19 6	−23
10	1 16 56	+8 17	1·42	1·00	19 24	−22
14	1 6 12	+7 46	1·46	1·01	19 41	−21
18	0 57 12	+7 19	1·51	1·02	19 58	−21
22	0 49 0	+6 58	1·55	1·03	20 15	−20
26	0 41 40	+6 37	1·59	1·06	20 32	−19
30	0 35 16	+6 22	1·64	1·09	20 49	−18
Feb. 3	0 29 36	+6 9	1·68	1·13	21 5	−17
7	0 24 28	+6 1	1·72	1·18	21 21	−15
11	0 19 48	+5 53	1·75	1·26	21 37	−14
15	0 15 28	+5 46	1·77	1·35	21 53	−13
19	0 11 24	+5 41	1·79	1·47	22 8	−11
23	0 7 32	+5 37	1·81	1·61	22 24	−10
27	0 3 48	+5 34	1·82	1·80	22 39	− 9
Mar. 3	0 0 0	+5 30	1·81	2·05	22 54	− 7
7	23 58 8	+5 26	1·80	2·37	23 9	− 6
11	52 24	+5 22	1·79	2·74	23 23	− 4
15	48 20	+5 16	1·75	3·28	23 38	− 2
19	23 44 0	+5 8	1·71	3·96	23 53	− 1
23	23 39 32	+4 58	1·65	4·83	0 7	+ 1
27	34 48	+4 47	1·58	5·94	0 22	+ 2

Greenwich Noon.	Right Ascension.	Declination.	Δ	Brightness.	Sun's Place. R.A.	Sun's Place. Dec.
1910.	h m s	° ′			h m	°
Mar. 31	23 30 8	+4 32	1·50	7·48	0 36	+ 4
Apr. 4	25 40	+4 14	1·40	8·95	0 51	+ 5
8	23 21 24	+3 55	1·28	10·86	1 6	+ 7
12	23 18 20	+3 35	1·15	13·06	1 20	+ 8
16	16 0	+3 16	1·02	16·22	1 35	+10
20	15 24	+3 1	·87	19·50	1 50	+11
24	17 16	+2 46	·72	25·12	2 5	+13
28	23 23 8	+2 37	·57	28·31	2 20	+14
May 2	23 35 44	+2 29	·41	58·34	2 35	+15
6	0 6 8	+2 37	·26	126·5	2 50	+16
10	2 2 40	+3 3	·08	1112·0	3 6	+17
14	7 21 40	+0 34	·10	660·7	3 22	+18
18	9 9 44	−0 41	·25	94·19	3 37	+19
22	9 44 8	−1 9	·40	33·27	3 53	+20
26	9 59 48	−1 21	·55	15·49	4 9	+21
30	10 9 40	−1 29	·70	8·59	4 26	+22
June 3	10 16 16	−1 37	·85	5·30	4 42	+22
7	10 21 4	−1 47	·99	3·51	4 58	+23
11	10 25 28	−1 56	1·15	2·42	5 15	+23
15	10 29 4	−2 5	1·30	1·74	5 32	+23
19	10 32 48	−2 14	1·42	1·34	5 48	+23
23	10 35 36	−2 29	1·58	1·00	6 5	+23
27	10 38 38	−2 41	1·71	·79	6 22	+23
July 1	10 41 38	−2 53	1·85	·63	6 38	+23
5	10 44 32	−3 8	1·98	·52	6 55	+23
9	10 47 28	−3 23	2·10	·42	7 11	+23
13	10 50 16	−3 38	2·22	·35	7 27	+21

Measures of Southern Binary Stars in 1907. By John Tebbutt.

I have pleasure in forwarding to the Society the results of a series of measures which I have made of the most interesting southern binary stars with the 8-inch equatorial, in the same manner as described in my paper in *Monthly Notices*, vol. lxvi. On the evenings of August 23, 24, 25 and September 15, 16, I attempted to measure the rapid binary Lacaille 7194, but failed to see the companion, although it was readily seen and measured by me in 1902. See *Monthly Notices* for November 1903. The non-visibility of the companion has been confirmed by observations subsequently made at my request at the Sydney Observatory.

Star.	Epoch.	Position Angle.	Distance.	No. of Evenings for Position Angle.	No. of Evenings for Distance.	Hour Angle.
α Centauri	1907·410	213°·8	20″·67	6	6	E.
β 416	·567	274°·2	2″·09	5	5	E.
γ Coronæ Aust.	·617	117°·3	1″·83	4	4	E.
h 5014	·652	...	1″·54	...	1	E.
,,	·660	240°·3	...	3	...	E.
γ Centauri	·673	353°·2	1″·42	5	2	W.
β Muscæ	·673.	346°·1	1″·28	5	2	W.
π Lupi	·704	84°·5	1″·42	7	3	W.
μ Lupi	·737	153°·3	1″·83	6	3	W.
γ Circini	·737	70°·2	1″·46	5	3	W.
d Lupi	·812	1°·4	...	2	...	W.
β Phœnicis	·875	12°·2	1″·55	5	3	E.
p Eridani	·907	216°·6	8″·23	3	4	E.
Lacaille 2145	·964	45°·2	1″·89	4	4	E.

The Observatory,
The Peninsula, Windsor, N.S. Wales:
1908 *January* 23.

On the relative numbers of Star Images photographed in different parts of the Plates for the Oxford portion of the Astrographic Catalogue. Second Paper. By H. H. Turner, D.Sc., F.R.S., Savilian Professor.

1. In *Monthly Notices*, lxii. p. 434, a preliminary discussion was given of the counts of star images in different parts of plates taken for the *Astrographic Catalogue*; and it was shown that the star density was greatest at a certain distance (r_0) from the centre of the plate and fell off rapidly outside this distance; the value of r_0 for different object-glasses being different,—*e.g.* for Oxford $r_0 = 33'$, for Paris $r_0 = 58'$, for Algiers $r_0 = 49'$, for Toulouse $r_0 = 30'$.

The natural inference is that the value of r_0 depends on the position of the plate, increasing as the plate is pushed further within the focus.

Similar results were found for the Potsdam telescope, with $r_0 = 28'$ (*Astron. Nachrichten*, No. 3817); and for the Greenwich telescope with $r_0 = 38'$ (*Greenwich Astrographic Catalogue*, vol. i., pp. vi and vii). On the other hand, the late M. Loewy has questioned the reality of the phenomenon in his annual report of the Paris Observatory for 1905, in the following terms:—

"D'autres expériences ont eu pour objet de déterminer jusqu'à quel point une même carte pouvait être considérée comme homogène au point de vue des grandeurs. On sait que M. H. H. Turner a été conduit à penser que cette homogénéité laissait à désirer, en faisant, pour un grand nombre d'épreuves publiées, le relevé des nombres d'étoiles qui tombent dans les divers carrés du réseau. Pour contrôler ce résultat par une autre voie, on a photographié un groupe d'étoiles avec des poses, décroissantes et de légers déplacements systématiques, afin d'obtenir des images faciles à identifier, et en même temps à la limite de visibilité. On a ensuite répété l'opération en imprimant à la lunette d'autres déplacements systématiques de manière à donner le même temps de pose aux mêmes étoiles dans diverses parties de la plaque. On a pu ainsi constater que la visibilité des images relatives aux astres les plus faibles est la même dans toutes les régions du champ. Le defaut signalé par M. Turner, et qui a pu se produire à la suite d'une mise au foyer défectueuse, ne semble donc pas très redoutable dans les circonstances actuelles" (p. 10).

One possible interpretation of the discrepancy between M. Loewy's conclusion and those above quoted is that his test was not so delicate a one as the numerical test elsewhere applied. The results obtained by counting star images are consistent and definite, and clearly afford a satisfactory measure of something. The fact that other methods fail to detect that something may therefore only enhance the value of the method which reveals it: there are almost certain to be uses to which a new test can be applied, even though they may not be foreseen at the moment. The quantity

measured has reference to the focal length of the instrument, and therefore to the scale of the photograph; and it is at any rate of interest to see how far such measures throw light on variations of focal length with time or with temperature; or on the inclination of the plate to the axis of the telescope (which may be regarded as variation in focal length for different parts of the plate).

2. Three volumes of the *Oxford Astrographic Catalogue* have now been published, containing measures of $3 \times 160 = 480$ plates, with centres in zones $+31^\circ$, $+30^\circ$, $+29^\circ$. Generous volunteer assistance

A B

2	10	26	50	82	122	170	226	290	362	442	530	626
10	18	34	58	90	130	178	234	298	370	450	538	634
26	34	50	74	106	146	194	250	314	386	466	554	650
50	58	74	98	130	170	218	274	338	410	490	578	674
82	90	106	130	162	202	250	306	370	442	522	610	706
122	130	146	170	202	242	290	346	410	482	562	650	746
170	178	194	218	250	290	338	394	458	530	610	698	794
226	234	250	274	306	346	394	450	514	586	666	754	850
290	298	314	338	370	410	458	514	578	650	730	818	914
362	370	386	410	442	482	530	586	650	722	802	890	986
442	450	466	490	522	562	610	666	730	802	882	970	1066
530	538	554	578	610	650	698	754	818	890	970	1058	1154
626	634	650	674	706	746	794	850	914	986	1066	1154	1250

8 16 24 32 40 48 56 64 72 80 88 96

C D

FIG. 1.

from Miss Riddle has enabled us to count the measures made on these plates on a systematic plan arranged so as to exhibit such variations in focal length. It seemed desirable to discuss these measures in order to decide whether to extend the work to the other four volumes as they appear; and to give a brief summary of the discussion, in order to enable our colleagues to judge of the expediency of undertaking similar work elsewhere.

3. Each plate was divided into four quarters, which will be denoted by XY, xY, Xy, xy. The capital letters refer to large values of x or y,—*i.e.* values exceeding 13·000; the small letters

to values less than 13·000. An alternative designation would thus be

$$XY = NE \quad xY = NW \quad Xy = SE \quad xy = SW$$

Each quarter was then divided up into ten regions, the average distances of which from the plate centre were in the ratios 1, 2, 3 . . . . 10, approximately, the boundaries of the regions following the reseau lines. The dividing lines for one of the quarters are shown in fig. 1. A represents the plate centre and D the corner of the reseau. Representing a side of the reseau by 2, the middle points of the reseau squares along A B will be at distances 1, 3, 5 . . . . 25. It is readily seen that on this scale the numbers written in the squares represent $r^2 = x^2 + y^2$, where x and y are the co-ordinates of the centre of the square referred to A as origin. Now if we take ten regions at distances t, $2t$, $3t$ . . . . $10t$ from A as centre, the value for the outermost will be $100t^2$; and a glance at fig. 1 shows that we must put approximately $100t^2 = 1100$, or t^2 = about 11.

TABLE I.

Letter for Region.	No. of Squares.	Average Value of r^2.	t^2.	r in Minutes of Arc.
a	3	7·3	7·3	7
b	5	28	7·0	13
c	14	76	8·4	22
d	15	150	9·4	31
e	23	248	9·9	39
f	23	363	10·1	48
g	34	508	10·4	56
h	29	673	10·5	65
i	15	864	10·7	73
k	8	1090	10·9	82

The thick lines were drawn with this value of t^2 in mind. The 4th and 5th columns of Table I. show that the grouping might have been better, but this was not noticed until so much work had been done that it seemed better to keep the system unaltered. It is not essential to have the distances in exact arithmetical progression.

4. The counts were then treated as shown in the following example:—

TABLE II.

Total counts in the regions for Plates 2034–2057, *Zone* +31°.

No. squares =	3	5	14	15	23	23	34	29	15	8	169
Region =	*a.*	*b.*	*c.*	*d.*	*e.*	*f.*	*g.*	*h.*	*i.*	*k.*	Total.
XY	25	30	101	137	222	276	372	297	132	64	1656
*x*Y	22	42	124	136	266	228	332	247	97	36	1530
X*y*	21	38	106	113	226	205	340	312	112	62	1535
xy	22	38	89	120	193	208	315	236	89	35	1345
	90	148	420	506	907	917	1359	1092	430	197	6066

The number of squares in a region varies considerably, as shown in the top line. The next procedure is to divide by this number, so as to get the average star density in each region. Then to reduce this density to a uniform scale for different batches of plates, the quotients are again divided by 6066/4, the average total number of stars on a single quarter-plate. The intermediate results need not be given here; in Table III. are shown the final values obtained for star density in the different regions.

TABLE III.

Star Density on a uniform scale deduced from Table II.

	a.	*b.*	*c.*	*d.*	*e.*	*f.*	*g.*	*h.*	*i.*	*k.*	Total.
XY	55	40	48	61	64	79	72	68	58	53	598
*x*Y	48	56	59	60	77	65	65	56	43	30	559
X*y*	46	50	50	50	65	59	66	72	50	52	560
xy	48	50	42	53	56	60	61	54	39	29	492
Mean	49	49	50	56	65	66	66	63	47	41	552

5. Now the characteristic feature of the variation in density is the gradual rise up to about *f* and *g* and the fall afterwards. The maximum represents the position of best focus, and it is important to note any variations in this position. For this purpose it is convenient to have a single number of some kind rather than a system of 10, and the indicator adopted for a preliminary investigation is

$$\Delta = (c + d + e) - (g + h + i).$$

It will be seen that an increase in Δ means that the maximum comes nearer *a*,—*i.e.* nearer the plate centre,—and presumably that the plate is further from the object-glass: a decrease in Δ corresponds to the plate being pushed in nearer the O.G.

To get an idea of the physical meaning of Δ, let us take the theoretical formula for star density on a plate given in *Mon. Not.*, lxii. p. 441, viz.—

$$\text{Density} = a\{1 \sim c(r^2 - r_0^2)\}$$

where r is the distance of the point on the plate from the plate centre; r_0 is the distance of the maximum from the centre; and $c = 10^{-4}$ if r and r_0 are expressed in minutes of arc. We may put r_0 successively equal to the selected distances in Table I.; and take $a = 66$, which is the mean maximum, on the system adopted in Table III.; and we then get the densities as in Table IV.

TABLE IV.

Theoretical Star Densities, with different positions of the maximum, and corresponding values of Δ and n.

r_0.	*a.*	*b.*	*c.*	*d.*	*e.*	*f.*	*g.*	*h.*	*i.*	*k.*	Δ.	*n.*
7′	66	65	63	60	56	51	45	38	31	22	+65	8039
13	65	66	64	61	57	52	46	39	32	23	+65	8202
22	63	64	66	63	59	54	48	41	34	25	+65	8508
31	60	61	63	66	62	57	51	44	37	28	+59	8883
39	56	57	59	62	66	61	55	48	41	32	+43	9263
48	51	52	54	57	61	66	60	53	46	37	+13	9508
56	45	46	48	51	55	60	66	59	52	43	−23	9526
65	38	39	41	44	48	53	59	66	59	50	−51	9071
73	31	32	34	37	41	46	52	59	66	57	−65	8210
82	22	23	25	28	32	37	43	50	57	66	−65	6833
No. of squares	3	5	14	15	23	23	34	29	15	8		

6. The values of Δ are shown in the last column but one of Table IV. When r_0 is small or large,—*i.e.* when the best focus is very near the centre or the corner of the plate,—Δ is constant, because the distances represented by $c\ d\ \ldots\ i$ are all on one side of the maximum, and hence $\Delta \equiv (c+d+e)-(g+h+i)$ does not change. But such cases do not occur in practice, the plate being always focussed for some distance from the centre, and then Δ is a convenient index to the position of best focus. In the last column is given, n, the total number of stars on the quarter-plate, obtained by assuming the density constant over each of the regions $a, b\ \ldots\ k$; so that the number of stars in each region is the product of the density by the number of squares in the region as shown in the last line of the table. The values of n so found imply the further assumption that the maximum density is the same (viz. 66 per square) whatever be the position of the plate. This assumption is no doubt incorrect, but it may not be far from the truth. The optical image of a star in focus near the axis of the lens is almost certainly smaller than the best focussed image of a star near the edge of the field. Hence, if the number of stars photographed depended on the size of best image alone, the maximum density would fall off as we left the centre of the field, and might fall off even rapidly. But other factors influence the photographic star image—atmospheric tremor, errors of driving and guiding, etc.—which are independent of the distance from the centre of the field, and the maximum density may not diminish so rapidly as might at first appear. It seems worth while to examine the consequences of assuming it to be constant, at any rate in the first instance. But we shall return to this point later. Our first business is with the *position* of the maximum, as indicated by the quantity Δ.

7. We proceed to tabulate the values of Δ for the four quarters,

arranging all the plates of vols. i., ii., iii. of the Oxford Catalogue in groups, as sufficiently indicated by columns 1, 2, 3, 4 in Table V. The detailed grouping was determined by considerations which need not concern us here; nor need we, in a preliminary survey, pay much attention to the fact that one or two of the individual groups are small either in number of plates or number of stars.

TABLE V.

Mean Values of Δ for Plates in vols. i., ii., iii. *of the Oxford Astrographic Catalogue.*

Zone.	Limiting Nos.	No. of Plates.	No. of Stars.	Values of Δ. XY.	*x*Y.	X*y*.	*xy*.	Whole.
29°	525– 542	16	8099	+48	+37	+42	+59	+46
29	700– 855	14	8589	+27	+34	+10	+28	+25
30	896– 921	15	4293	+25	+42	+51	+17	+34
29	937–1023	32	7300	+51	+49	+52	+15	+42
29	1236–1486	18	7913	+52	+12	+23	+37	+31
Totals and Means		95	36194	+41	+35	+36	+31	+36
30	1564–1585	11	12593	−28	− 5	−44	+15	−16
30	1586–1597	12	5933	−49	− 4	−34	+30	−14
31	1598–1606	8	4877	−76	+ 7	−81	+ 3	−37
Totals and Means		31	23403	−51	− 1	−53	+16	−22
29	1747–1843	20	6706	− 1	+51	−20	+34	+16
31	1834–1849	7	6245	−37	+ 3	−38	−21	−23
29	1850–1900	26	8860	+ 2	+37	−30	+34	+11
30	1865–1915	14	8178	− 6	+38	−30	+27	+ 7
30	1916–1943	26	5989	− 1	+48	−22	+38	+16
Totals and Means		93	35978	− 9	+35	−28	+22	+ 5
30	1946–2060	26	11217	−30	+32	−56	+ 8	−11
31	1973–1996	11	4006	−22	+30	−59	−11	−16
31	1997–2032	26	19984	−29	+37	−64	+ 1	−14
31	2034–2057	24	6066	−25	+32	−23	− 3	− 5
Totals and Means		87	41273	−27	+33	−50	− 1	−11
30	2061–2101	26	7797	+ 1	+12	−30	+42	+ 6
31	2087–2155	15	4513	−39	+46	− 9	+34	+ 8
30	2102–2111	10	2693	0	+29	−27	+17	+ 5
30	2136–2196	16	6726	−13	+25	−47	+26	− 2
31	2202–2234	15	4445	−14	+24	− 1	+40	+12
30	2217–2482	4	1163	+12	−16	−28	+56	+ 6
31	2235–2271	26	6095	− 5	+47	−22	+29	+12
31	2274–2299	12	4871	−33	− 6	−41	+25	−14
Totals and Means		124	38303	−11	+20	−26	+34	+ 4
31	2311–2349	18	4596	+ 2	+21	− 5	+43	+15
29	2450–2500	30	11874	−23	+25	−22	+23	+ 1

8. The reasons for separation at the places indicated by the horizontal lines are as follows:—

(*a*) Between plate 1557 and 1558 (summer of 1900) the new dome was erected. The object-glass was dismounted and the eye end also.

(*b*) After plate 1606, the instrument was devoted to the Eros work. The eye end was taken off to study possible instrumental adaptations.

(*c*) After plate 1943 the O.G. was dismounted for cleaning.

(*d*) Some change seems to have taken place between 2057–2061, or near this date, though nothing is recorded in explanation.

(*e*) At plate 2300 the eye end was dismounted to study adaptation to photographs of the Moon among the stars.

(*f*) After plate 2349 the O.G. was dismounted and taken to Egypt for the eclipse of 1905.

There was apparently a slight change of focal length at each of these epochs, due to accidental causes. It is curious that, on the whole, the value of Δ should show an *increase* from the second group onwards, for this corresponds to the plate being further from the O.G., and not nearer to it, as might have been explained by the gradual wearing of the studs against which it is pressed. No change was made deliberately in the adjustment of these until after all the plates had been taken.

9. It is a reasonable assumption that on each occasion the focal length was changed without disturbing the relationship of the quarters to each other. Hence we may subtract the value of Δ for the whole plate (given in the last column) from each quarter. It will be sufficient here to give the means of groups.

TABLE VI.

Values of Δ *for each quarter relative to mean* Δ *for Plate.*

	Limiting Nos.	Weight.	XY.	*x*Y.	X*y*.	*xy*.
I.	525–1486	5	+ 5	− 1	0	− 5
II.	1564–1606	3	−29	+21	−31	+38
III.	1747–1943	5	−14	+30	−33	+17
IV.	1946–2057	5	−16	+44	−39	+10
V.	2061–2299	5	−15	+16	−30	+30
VI.	2311–2349	1	−13	+ 6	−20	+28
VII.	2450–2500	2	−24	+24	−23	+22

There is a decided change after the first group. In the first group the quarters XY and X*y* are slightly positive, corresponding to the X side of the plate (large R.A.*s*) being a little further from the O.G. than the *x* side. In all the later groups the reverse is the

case, and the relationship remains fairly constant. It is true there seems to be a slight diminution in xY and an increase in Xy, but the appearance is chiefly due to the last two groups, which have small weight.

10. The changes in general focal length seem more striking than any relative change. But it is possible to take another view of these changes. Are they possibly seasonal? No deliberate change in focus was made at any of the epochs above mentioned, and at one of them no interference with the instrument is recorded.

11. Let us try the extreme assumption that they are all due to temperature or other seasonal change. In the third column of Table VII. is given the month in which the majority of the plates in the group were taken. Arranging the groups according to these months we have—

Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
+6	+7	−2	+16	+12	−11	+1	−16	−14		− 5	
	+8		+16		−16		−14	−37		+11	+15
	+5		+12		−14			−23			
			+ 6								

There certainly seems to be something to be said for a seasonal effect. The values of Δ from December to May are all positive with one slight exception, and from June to November all negative. If we express the effect as a simple harmonic term, with a constant added, we should have figures something like those given below :—

Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
+4	+10	+12	+10	+4	−4	−12	−18	−20	−18	−12	−4

Applying these as corrections with reversed sign to the individual groups, we get the results of Table VII.

TABLE VII.

Possible seasonal change in Δ.

	Uncorrected Δ.	Month.	Correction.	Corrected Δ.
1564–1585	−16	Aug.	+18	+ 2
1586–1597	−14	Sept.	+20	+ 6
1598–1606	−37	Sept.	+20	−17
Mean	−22	...	+19	− 3
1747–1843	+16	Apr.	−10	+ 6
1834–1849	−23	Sept.	+20	− 3
1850–1900	+11	Nov.	+12	+23
1865–1915	+ 7	Feb.	−10	− 3
1916–1943	+16	Apr.	−10	+ 6
	+ 5	...	+ 1	+ 6

TABLE VII.—*continued.*

Possible seasonal change in Δ.

	Uncorrected Δ.	Month.	Correction.	Corrected Δ.
1946–2060	−11	June	+ 4	− 7
1973–1996	−16	June	+ 4	−12
1997–2032	−14	Aug.	+18	+ 4
2034–2057	− 5	Nov.	+12	+ 7
	−11	...	+10	− 2
2061–2101	+ 6	Jan.	− 4	+ 2
2087–2155	+ 8	Feb.	−10	− 2
2102–2111	+ 5	Feb.	−10	− 5
2136–2196	− 2	Mar.	−12	−14
2202–2234	+12	Apr.	−10	+ 2
2217–2482	+ 6	Apr.	−10	− 4
2235–2271	+12	May	− 4	+ 8
2274–2299	−14	June	+ 4	−10
	+ 4	...	− 7	− 3
2311–2349	+15	Dec.	+ 4	+19
2450–2500	+ 1	July	+12	+13

The figures can thus be explained either by a seasonal change in focal length, or by a series of discontinuities at the times when either eye end or O.G. were dismounted.

Against the latter explanation there is the fact that no discontinuity was recorded about plate No. 2057, so that the seasonal effect is somewhat more probable.

The first set of plates (Nos. 525–1486) throws no light on the matter, for the most divergent groups in the last column of Table IV. are the first two, which were both taken chiefly in September, so that we must set down their difference to accident.

Summarising the results so far, we find—

(α) That the focal position of the plate was essentially different in the two periods before and after the erection of the new dome in 1900.

(β) In the first period the plate was further from the object-glass ($\Delta = +36$ for the mean of the four quarters), and satisfactorily normal to the line of collimation (values of Δ nearly same in all quarters).

(γ) In the second period the plate was nearer the O.G. ($\Delta = 0$ or less), and the X side (large R.A.'s) much nearer than the x side (small R.A.'s). The values of Δ for the two sides are about $\Delta = -25$ and $\Delta = +25$ respectively.

Moreover, there are variations in the focal length which may be seasonal, ranging from $\Delta = -20$ in June–September (plate

nearer O.G.) to $\Delta = +12$ in December–April (plate further from O.G.), though these variations may be due to a series of discontinuities.

The result that the plate is further from the O.G. in cold weather and nearer in warm seems a little strange. The telescope tube is no doubt longer in the warm weather; but we are concerned with a differential effect, depending partly on the expansion of the tube and partly on that of the lenses, which may alter their focal length more than the tube alters, and so give an apparently reversed effect. For this reason we cannot look for confirmation (or otherwise) to the observed scale-value of the plate deduced from measures of star images; for this scale value depends on the expansion of tube and plate, perhaps also on that of the reseau.

12. Let us now examine the effect of these differences on the relative number of stars photographed in the different quarter-plates. We find that we have in plates 1564–2500 a mass of tolerably homogeneous material with one chief variable, the mean value of Δ for the whole plate, which probably corresponds to the mean distance of the plate from the O.G. Whether the variations in Δ are due to a seasonal effect or to accidental discontinuities need not concern us if we take Δ itself as the independent variable.

TABLE VIII.

Total number of Stars counted on each Quarter-Plate.

Mean Δ.	Plate Nos.	Zone.	XY.	*x*Y.	X*y*.	*xy*.
−37	1598–1606	31°	1320	1208	1269	1080
−23	1834–1849	31	1528	1571	1582	1564
−16	1564–1585	30	3175	3332	3161	2925
−16	1973–1996	31	1022	965	1070	949
			7045	7076	7082	6518
−14	1586–1597	30	1582	1540	1462	1349
−14	1997–2032	31	5201	5075	5000	4708
−14	2274–2299	31	1289	1187	1266	1129
−11	1946–2060	30	2951	2743	2901	2622
− 5	2034–2057	31	1656	1530	1535	1345
− 2	2136–2196	30	1820	1525	1813	1568
+ 1	2450–2500	29	3247	3011	2874	2742
			17746	16611	16851	15463
+ 6	2061–2101	30	1972	1891	2019	1915
+ 5	2102–2111	30	752	653	675	613
+ 6	2217–2482	30	314	293	298	258
+11	1850–1900	29	2308	2190	2250	2108
+ 7	1865–1915	30	2290	2023	2101	1764
+ 8	2087–2155	31	1193	1174	1130	1006
			8829	8224	8473	7664

TABLE VIII.—*continued.*

Total number of Stars counted on each Quarter-Plate.

Mean Δ.	Plate Nos.	Zone.	XY.	*x*Y.	X*y*.	*xy*.
+12	2235–2271	31°	1560	1494	1598	1443
+12	2202–2234	31	1185	1103	1147	1010
+16	1747–1843	29	1679	1630	1755	1642
+15	2311–2349	31	1188	1194	1099	1115
+16	1916–1943	30	1647	1449	1527	1366
			7259	6870	7126	6576

Reducing the numbers to percentages we find for the means of groups—

TABLE IX.

Mean Δ.	XY.	*x*Y.	X*y*.	*xy*.
−23	101·6	102·1	102·2	94·1
− 8	106·4	99·7	101·1	92·8
+ 7	106·4	99·1	102·0	92·3
+14	104·3	98·7	102·4	94·5
Mean	104·7	99·9	101·9	93·4

13. We have now to correct these numbers for the changes in Δ. Table IV. gives in the last two columns the values of n (the whole number of stars on a quarter-plate) for different values of Δ on certain assumptions specified in § 6. We may make a small table of the *percentage* correction to n as follows :—

Δ =	+ 60	+ 50	+ 40	+ 30	+ 20	+ 10	0	− 10	− 20	− 30	− 40	− 50	− 60
Corrn. to n p. c. =	+7·5	+4·5	+2·8	+1·7	+0·9	+0·3	+0·1	0·0	+0·1	+1·0	+2·5	+4·5	+9·7

Now the mean values of Δ for the groups of Table IX. and for each quarter-plate, and the consequent corrections to percentage, would be as follows :—

TABLE X.

Group (Table IX.)	Mean Value of Δ.				Correction to Percentage.				Mean.
	XY.	*x*Y.	X*y*.	*xy*.	XY.	*x*Y.	X*y*.	*xy*.	
I.	−41	+ 9	−55	− 4	+2·7	+0·2	+7·1	0·0	+2·5
II.	−27	+20	−41	+13	+0·7	+0·9	+2·7	+0·4	+1·2
III.	− 5	+24	−26	+34	0·0	+1·2	+0·5	+2·1	+0·9
IV.	− 4	+38	−14	+36	0·0	+2·7	0·0	+2·4	+1·3

Subtracting the mean shown in the last column from the separate corrections for each quarter-plate (so as to keep the mean for the whole-plate zero), the numbers of Table IX. corrected would then be as in Table XI.

TABLE XI.

Table IX. *corrected to uniform* $\Delta = -10$.

	XY.	xY.	Xy.	xy.
	101·8	99·8	106·8	91·6
	105·9	99·4	102·6	92·0
	105·5	99·4	101·6	93·5
	102·9	100·1	101·2	95·5
Mean	104·0	99·7	103·1	93·2

14. It is clear that we have not removed the chief part of the differences shown in Table IX. by this process, and we must look to other causes for them. One such cause is undoubtedly *tilt* of

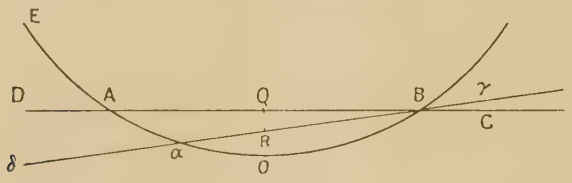


Fig. 2.

the plate, of which no account has yet been taken in dealing with the number of stars as distinct from the position of best focus. That the tilt seriously affects the number of stars can be seen from elementary geometrical considerations. Let E A O B (fig 2) represent a section of the curved field and D A B C a position of the plate normal to the axis.

Now if the plate be tilted to the position $\delta\,\alpha\,B\,\gamma$, it is clear that every portion of the half Q C or R γ is brought *closer* to the curve of good focus, and this side of the plate will thus contain more stars all over. On the other side, regarding the plate as first moved parallel to itself to cut the curve at α, and then tilted, we have in what precedes taken account of the movement of translation, but not of the tilt; and the tilt clearly moves every part of this other half-plate *further* from the curve of good focus, so that we lose stars all over it. The effect of tilt is thus greater than that of translation, because by moving the plate parallel to itself we gain in one part and lose in another, whereas by tilt we either gain or lose all over the half-plate. We must qualify this statement a little when we extend the argument to two dimensions, so as to deal with a curved surface instead of the curved arc shown in the figure. But the general nature of the phenomenon remains; and we see that *when a plate is tilted, the side nearer the O.G. gains at the expense of the side more remote.*

15. We thus see how parts of the large differences of Tables IX. and XI. probably arise. The quarter-plates XY and Xy, which have Δ negative, are nearer the O.G. and gain stars

from their opposites xy and xY. But it is not necessary to proceed to a quantitative estimation to see that we cannot in this way explain the whole of the differences. There must be some other contributing cause which makes the Y quadrants 4 per cent. richer in stars than the y quadrants. The difference between the mean Δ for the Y quadrants and the y quadrants is so slight that tilt cannot explain this considerable excess; and, while reserving the study of tilt for a future paper, we may proceed here briefly to consider the possible causes of an excess of stars in the north half of a plate which cannot be explained (so far as can be seen at present) by the position of the plate.

16. The excess of stars in the N half of the plate may be due to any combination of the following causes:—

(α) Optical performance of the O.G., including possible inclination to the line of collimation.

(β) Increased atmospheric absorption for S stars.

(γ) Actual increase in number of stars as we go northwards. When we combine all the plates in a zone, we eliminate (to a large extent) variations of uniformity in R.A., but a change with declination may be persistent.

17. It is possible to estimate the approximate magnitude of cause (β). The formula for atmospheric absorption at Oxford of visual rays is given in *Mem. R.A.S.*, vol. xlvii., as 0·25 sec. Z.D. We may put Z.D. = 25°; and the variation of sec. Z.D. for 1°, which is the distance between N and S halves of the plate, is about 0·01. The correction is thus about ·0025 magnitude. Now Newcomb gives (in his book, *The Stars: a Study of the Universe*, p. 283) the number of stars of different magnitudes as in the second column of Table XII., whence we get the totals of that magnitude and brighter as in the third column, from which we

Table XII.

Mag.	No. of Stars.	Total = N.	log N.	Diff.
6·5	2	2	0·30	·30
7·0	2	4	0·60	·30
7·5	4	8	0·90	·38
8·0	11	19	1·28	·25
8·5	15	34	1·53	·27
9·0	29	63	1·80	·18
9·5	33	96	1·98	·15
10·0	39	135	2·13	·17
10·5	64	199	2·30	·20
11·0	115	314	2·50	

form log N and its differences as in the 4th and 5th columns. It appears that at magnitude 11, log N is increasing at the rate

of about ·4 per magnitude, so that the increase in log N for ·0025 mag. would be about ·001. This would increase 100 stars to 100·23 (since log 100·23 = 1·001), so that a difference of atmospheric absorption indicated by visual observations would only make a difference of 0·23 per cent. between the upper and lower halves of a plate. Photographic absorption would need to be twenty times as large to explain the observed difference, which is not likely. Sir W. Abney emphasises (*Mon. Not.*, xlvii. p. 265) the necessity for knowing the "spectrum value" of the plates employed if we wish to determine the photographed absorption, but the limits between which it varies are not likely to be wider than from one to four times the visual absorption.

18. It occurred to me that we might get some indication of the photographic absorption by comparing in some way the plates taken at different hour-angles; for instance, in the formula given at the heading of each of our plates for determining magnitude from measures of diameters,

$$\text{mag.} = a - b\sqrt{d},$$

the constant a would vary with the atmospheric absorption. But a little examination showed that the differences of Z.D. between plates taken otherwise under the same conditions were too small to afford a trustworthy indication of the phenomenon, which is swamped by a number of other larger variations. A single example will serve to show this. In Table XIII. are shown in the first three columns the Decl. and R.A. of the plate centre and the hour-angle at the middle of the exposure; in the fourth column is given the Z.D. of the centre; and in the fifth the value of the constant a of the above formula. If these values of a are arranged according to Z.D. we find—

No. Plates.	Mean Z.D.	Mean a.
7	35°·4	16·43
6	41°·0	16·78

i.e. the paradoxical result that fainter magnitudes (by 0·38 magnitude) are shown on the plates of 6° greater Z.D. Suspicion arose that the person who measured the plate might influence the result; and on collecting the results for the three measurers shown in the sixth column, it was found that their mean values of a were

BG.	EG.	S.
$16{\cdot}84_5$	$16{\cdot}40_5$	$16{\cdot}50_3$

TABLE XIII.

Plates taken on the night of 1903 *October* 30.

Decl.	R.A.	H.A.	Z.D.	*a.*	Measurer.	Corrn.
+31°	3·31	1·39 E	28°	16·2	EG	+·2
,,	5·55	3·41 ,,	45	16·5	EG	+·2
,,	6· 4	3·30 ,,	43	17·7	BG	−·2
,,	6·13	3·13 ,,	41	17·0	BG	−·2
,,	6·22	3· 3 ,,	39	16·3	BG	−·2
,,	6·58	3·10 ,,	40	16·3	S	+·1
,,	7· 7	3· 0 ,,	39	16·9	S	+·1
,,	7·25	2·57 ,,	38	16·6	EG	+·2
,,	7·34	2·41 ,,	36	16·8	BG	−·2
,,	8· 1	2·49 ,,	37	16·4	BG	−·2
,,	8·28	2·55 ,,	38	16·2	EG	+·2
,,	8·46	2·39 ,,	36	16·3	S	+·1
,,	8·55	2·31 ,,	35	16·5	EG	+·2

but on applying corrections as in the last column the 16·43 only became 16·50 and the 16·78 became 16·75, so that the anomaly was not removed. It seems better to determine the photographic absorption by special observations rather than to seek it in the plates already taken, in which its effect was consciously or unconsciously minimised.

19. We may now turn to cause (γ), the distribution of stars in the sky. That the density increases as we go northwards in the Oxford zones is shown, for instance, in Argelander's *Durchmusterung*. His numbers per square degree for zones +28°, +29°, +30°, +31°, are 14·9, 16·2, 16·4, 16·4; so that the differences per cent. between the north half and the south half of plates in zones with centres +29°, +30°, +31°, here discussed, would be 8·1, 1·3, 0·0; mean, 3·1. This is comparable with our 4·0 per cent., and the difference is what we might expect, considering that Argelander only goes to magnitude 10. But it is unsatisfactory that the difference is chiefly in one zone. We may put this down to accidental error, but the evidence is only sufficient to state a case for inquiry. In *Monthly Notices*, vol. lx., plate 2 (opposite p. 16), Mr. Bellamy has given a curve showing the approximate variation in stellar density (for the Oxford zones) with Galactic latitude somewhat as follows:—

TABLE XIV.

Gal. Lat.	0°.	10°.	20°.	30°.	40°.	50°.	60°.	70°.	80°.	90°.
N = No. of stars	800	460	310	230	180	155	135	120	110	105
Differences		340	150	80	50	25	20	15	10	5
Log N	2·90	2·66	2·49	2·36	2·26	2·19	2·13	2·08	2·04	2·02
Differences		0·36	0·17	0·13	0·10	0·07	0·06	0·05	0·04	0·02

20. There are two important points lying away from the curve, but we will neglect these in the following brief and general remarks. The Galaxy cuts the parallel of 30° Dec. about $5\frac{1}{2}^h$ and $19\frac{1}{2}^h$.

(A) At these crossing points the changes of stellar density are very large, but *if they are symmetrical* on opposite sides of the Galaxy there should be a rough compensation. On one side the N half of a plate will be richer, on the other the S half. We must be prepared, however, to find this compensation break down if the Milky Way is unsymmetrical.

(B) In the neighbourhood of 0^h the Oxford zones are near the Milky Way, south of it, and nearly parallel to it. Thus to pass from the southern half of a plate to the north is to approach the Milky Way, and consequently to find more stars. In these R.A.'s, therefore, we may expect to find the main contributions to the discrepancy between N and S halves of a plate. What is the maximum contribution we can expect from them? Suppose they ran all the way from 20^h to 5^h (9 hours of R.A. out of the 24), parallel to the Milky Way and 15° from it. We see from Table XIV. that log N increases 0·17 in 10° of Galactic latitude, or 0·017 in 1°; so that 100 stars on the S half of a plate would become 104 on the N half, due to approach to the Galaxy. This is an increase of 4 per cent., which is about what is required; but it becomes inadequate when we take 9/24 of it in order to distribute it round the whole zone. This fraction 9/24 corresponds to uniform distribution in R.A., which is, of course, not in strict accordance with the facts; but weighting each hour by the number of stars in it gives a nearly equal fraction, since the rich Galactic portions are chiefly in the non-contributing R.A.'s.

(C) For the remainder of the zone contributes nothing, *or goes the other way.* We have spoken of the crossing points in (A) above; from R.A. 5^h–6^h, for instance, the increase northwards is rapid for the first half hour, but is balanced by an equally rapid diminution when the central line is crossed. Reserving for future investigation the effects of possible asymmetry, we may put aside this crossing region as contributing nothing. Proceeding to greater R.A.'s, we are now north of the Galaxy, and thus stars decrease in number as we go northwards. The quantity N–S will thus be negative. But the zone +30° is a small circle of the sphere, and presently its sharper curvature brings it perpendicular to the Galaxy, when the difference N–S will be zero, and then positive again. It is positive for about an hour only (near $12\frac{1}{2}^h$) and then is negative again up to 19^h (say), when we get the other crossing point (19^h–20^h).

21. These facts can, of course, be expressed in tabular form, and are given below in Table XV.; but it seemed desirable to call attention to them also in general terms, because general considerations seem to show that we cannot fairly expect, by improved knowledge of the details, to explain the whole of the difference N–S by actual distribution of the stars in Galactic latitude alone. The figures given below are only approximate: they could doubt-

less be improved, for instance, by using Professor Kapteyn's results in No. 18 of his publications, which have become available by his kindness in sending an advance copy since Table XV. was constructed. But the change would not be great. One of the most important details left outstanding is that of the symmetry or want of symmetry of the Galaxy on opposite sides; but Professor Kapteyn's paper does not touch this point, since he has not yet undertaken any discussion of distribution in Galactic longitude.

TABLE XV.

R.A.	Gal. Lat. (Zone +30°).	Calcd. N–S. per cent.	Observed (N–S) per cent.			Mean.
			31°.	30°.	29°.	
0·5	−33°	+3	+ 8	− 6	− 3	0
1·5	−31	+3	+10	−12	+13	+ 4
2·5	−27	+3	+ 8	0	+ 8	+ 5
3·5	−20	+3	+24	−17	+18	+ 8
4·5	−11	+4	+ 8	+30	+34	+24
5·5	0	...	+ 3	+21	+24	+16
6·5	+11	−2	0	−10	− 2	− 4
7·5	+23	−1	+15	− 4	− 5	+ 2
8·5	+35	0	+ 9	− 6	+ 4	+ 2
9·5	+48	0	+ 5	− 1	− 5	0
10·5	+61	0	+ 5	− 3	+ 9	+ 4
11·5	+74	0	+ 4	+10	− 2	+ 4
12·5	+86	+1	+ 4	− 5	0	0
13·5	+79	0	0	+ 3	− 2	0
14·5	+66	0	− 4	+ 5	+ 6	+ 2
15·5	+53	0	+ 8	+11	−11	+ 3
16·5	+40	−1	− 7	+10	+22	+ 8
17·5	+28	−1	+ 1	− 1	+ 3	+ 1
18·5	+16	−2	+ 7	+ 7	+ 3	+ 6
19·5	+ 4	...	+ 2	− 4	+18	+ 5
20·5	− 7	+3	+ 1	+15	+14	+10
21·5	−16	+3	+11	+ 4	+ 9	+ 8
22·5	−25	+3	+ 2	+12	+ 9	+ 8
23·5	−30	+3	+15	+11	+ 8	+11

22. Percentages are given in Table XV. rather than total numbers of stars, in order to avoid undue influence from the Galaxy. It will be seen that the observed percentage is from $20^{h}\cdot5$ to $23^{h}\cdot5$ +9, and from $0^{h}\cdot5$ to $4^{h}\cdot5$ it is +8; the mean for the whole 9 hours being +8·7, as against a theoretical +3·1. For the portion $6^{h}\cdot5$ to $18^{h}\cdot5$ the observed N–S percentage is +2·2, as against a theoretical −0·5. Hence the mean observed N–S is throughout in excess of the theoretical; or, in other

words, only a part of it can be set down to star distribution; the rest must be due either to atmospheric absorption or unsymmetrical performance of the object-glass. But these latter causes do not vary with the R.A. If we estimate their combined effect as 2 or 3 per cent. (being guided by the figures for $6^h{\cdot}5$ to $18^h{\cdot}5$, where the effect of stellar distribution is small), we get 6 or 7 per cent. for the observed effect of stellar distribution in R.A. $20^h{\cdot}5$ to $4^h{\cdot}5$ instead of about 3. This suggests that the rate of diminution with Galactic latitude has been considerably underestimated in those regions. A scrutiny of the figures shows that this is quite possibly the case, the reason being that the rate of diminution varies considerably in different Galactic longitudes. But a complete discussion of this point cannot be given here.

Summary.

§§ 1, 2. Introductory.

§§ 3, 4. Method of measuring the average star density on each quarter-plate, at different distances from the centre.

§ 5. Definition of Δ, which indicates the distance of maximum density from the centre. Table of values of star density and total number of stars for different values of Δ.

§ 7. List of observed values of Δ, in order of date of exposure of plates, in vols. i., ii., and iii.

§§ 8–11. Variations of focal length indicated by changes of mean Δ may be due to discontinuous disturbances of the plate, but are more probably due to seasonal changes in focal length.

But there is certainly one discontinuity, at the erection of the new dome in 1900. Before this the plate was satisfactorily normal to the telescope axis; afterwards it was apparently tilted in R.A., greater R.A.'s being nearer the O.G.

§§ 12–14. In consequence of this tilt, the side of greater R.A. is richer in stars; but the detailed study of the effect of tilt is reserved for a separate investigation.

§ 15. But there is an excess of stars in the N half of the plate, which is 4 per cent. richer than the S half, and this cannot be due to tilt.

§§ 17, 18. Visual atmospheric absorption would give an excess of 0·23 per cent. in the N half: photographic absorption may be greater, but its value cannot be well determined from the Oxford plates, which were taken at nearly constant Z.D. A special investigation of it is desirable.

§§ 19–21. The distribution of the stars in Galactic latitude alone explains a part, but cannot explain the whole of the 4 per cent. excess in the N half of the plates. The general nature of the distribution in Galactic longitude is such as to explain another portion of the excess, but quantitative estimates cannot yet be made.

Further Considerations on the Correlations of Stellar Characters. By Winifred Gibson, B.Sc., formerly Jessel Scholar, University College, London, and Karl Pearson, F.R.S., University College, London. (With Six Diagrams.)

(1) *Introductory.*—In a paper communicated to the Society last year (*Monthly Notices*, vol. lxvi. p. 445), modern statistical methods were used for the first time to determine the numerical relationships between various star characters. The object of the present paper is to deduce further similar relationships, and to deal with some of the same relationships on the basis of wider data. The general characters with which we have to deal, and which are more or less accurately known for larger or smaller stellar populations, are (1) magnitude, (2) colour, (3) spectral class, (4) proper motion, (5) parallax, (6) position. In any attempt to look upon the stellar universe as an orderly whole, the relationships between these characters must be of fundamental importance. To determine their numerical values is the first stage by which we pass from chaos to an organised and locally differentiated cosmos. The aid which the statistician may venture to offer the trained astronomer in this respect may, perhaps, be illustrated by reference to some recent work. Since the publication of the first paper above referred to, two memoirs, both of considerable importance, have appeared. The first is that of Messrs. Chase, Smith, and Elkin ("Parallax Investigations on 163 Stars, mainly of large Proper Motion," *Transactions, Yale Univ. Observatory*, vol. ii. pp. 1–207). This memoir deals with the relationship of proper motion, magnitude, parallax, and spectral class, among much else of great value, but not bearing on the topics we have at present in hand.

A second memoir of less scope but of considerable interest is that of Mr. W. S. Franks ("The Relation between Star Colours and Spectra," *Monthly Notices*, vol. lxvii. pp. 539–42). Now, these memoirs more than suffice to show that the distribution of star characters is not one of mere random association. The characters occur in a *correlated* manner, and this in itself is suggestive of the cosmos being a differentiated organisation. Even if we at once admit that we might anticipate that parallax would be related to proper motion, or even to magnitude, or, again, that colour and spectral group would be found in association, it is less obvious that spectral class will be found related to magnitude, proper motion, and parallax.

Yet, even when we see these relationships indicated in the above and other memoirs, there appears to be something lacking, which it is, perhaps, possible for modern statistical methods to supply. It would not be possible from the above type of classificatory work to determine the intensity of the relationship between the characters under consideration. For example: Is parallax more closely associated with magnitude or with spectral class? Or, again: Is magnitude more closely related to distance

than to chemical composition? To say that the latter is nearly four times as influential as the former is to crystallise at once our general ideas on magnitude.

Accordingly, it seems possible that modern statistical methods may be of some aid in determining the intensity of relationship between various stellar characters; in appreciating what, to adopt a term from biometry, may be spoken of as the organic correlations of the population. It is not suggested that, any more than in the science referred to, the individual must be lost sight of in a cloud of average relationships. But a knowledge of the extent to which stellar characters are correlated may, if properly used, be helpful in indicating the directions of profitable further analysis.

(2) *Determination of Correlation.*—It may not be out of place here to give a brief summary of the constants by aid of which correlation is determined in modern statistical practice. This is the more important as it is desirable to indicate the limits of their proper application in the case of stellar characters. The methods in use are threefold:—

(i) *Coefficient of Correlation*, usually represented by r. This can only be used effectively if both characters are quantitative and actually measured. Let A and B be the two characters, m_1 and m_2 their mean values, m_1+x, m_2+y, the characters in any pair of individuals, σ_1 and σ_2 the standard deviations (square roots of mean square deviations of either group of characters); then if n be the size of the sample taken of the population,

$$r_{12} = \mathrm{S}(xy)/(n\sigma_1\sigma_2) \quad . \quad . \quad . \quad . \quad (i)$$

v possesses the following properties: it lies between -1 and $+1$, according to the intensity of the relationship; $r_{12}\sigma_1/\sigma_2$ is the slope of the best fitting straight line of the average values of A for a given value of B; and $r_{12}\sigma_2/\sigma_1$ is the slope of the corresponding line for average values of B for given values of A. $\sigma_1\sqrt{1-r_{12}^2}$ is the average standard deviation of arrays of A for given values of B, and $\sigma_2\sqrt{1-r_{12}^2}$ is the average deviation from the straight line of arrays of B for given values of A. When the average values of one character for a given value of a second lie nearly on a straight line the correlation is said to be linear, and in this case the vanishing of r_{12} marks the absolute independence of the two characters. r_{12} is always the same as r_{21}.

All this is completely independent of the nature of the frequency distribution. Provided the characters are quantitatively measurable and the correlation is approximately linear, the correlation coefficient r is pre-eminently the best suited to express the degree of interdependence of the two variables.

For example, it was used in the first paper in determining the relationship between proper motion in R.A. and in declination.

(ii) *The Correlation Ratio*, usually represented by η. If the curve of mean values of A for given values of B depart widely from a straight line, then the vanishing of r does not necessarily mean that

the characters A and B are unrelated. It would only signify that the best fitting straight line was horizontal. In order to cover this case the correlation ratio η has been introduced. Let Σ_1 be the standard deviation of the means of arrays of A for given values of B, each array mean being weighted with its total frequency; then,

$$\eta_{12} = \Sigma_1/\sigma_1 \quad . \quad . \quad . \quad . \quad \text{(ii)}$$

η possesses the following characters: it lies between 0 and 1, being always zero if the characters are independent, and unity if they are absolutely related or causal. η always lies between r and 1 and is equal to r when the correlation is linear. $\sigma_1 \sqrt{1-\eta_{12}^2}$ is the mean standard deviation of arrays of A for a given B. $\sigma_1 \sqrt{\eta_{12}^2 - r_{12}^2}$ is the mean square deviation of the curve of means from its best fitting straight line. η_{12} is not necessarily equal to η_{21}. When the correlation is approximately linear, we have $\eta_{12} = \eta_{21} = r_{12}$ very closely, and this relationship holds for a very wide range of physical and organic variables.

η, the correlation ratio, will clearly be of service when one variable, say B, is not quantitative, but classificatory, because in this case we can determine Σ_1 and σ_1 although we have no quantitative measures of B. We ought, however, to have fairly fine groupings of B. Such cases are those of stellar spectra and stellar colour. On the other hand, η_{12} and η_{21} may be found in order to measure the degree of divergence of the correlation from linearity. As illustration of this, we may consider magnitude. If we replace it by amount of light, this might possibly give a true quantitative scale; but not only does the reduction involve some doubt, but it introduces extremely laborious calculations compared to the simplicity of magnitude proper. Accordingly the η method, classifying according to magnitude, seems a suitable method of approaching the problem.

(iii) *Coefficient of mean square Contingency*, usually represented by C_1.

Let the arrangement of any table of two variables—*e.g.* Mr. Franks' tables of colour and spectral class—be purely classificatory. Let the frequency of any class a of A in the population N be n_a and of the class b of B be n_b; let the frequency of individuals combining both classes be n_{ab}, then

$$\phi^2 = \mathrm{S}\left\{\left(n_{ab} - \frac{n_a n_b}{\mathrm{N}}\right)^2 \Big/ \frac{n_a n_b}{\mathrm{N}}\right\} \frac{1}{\mathrm{N}}. \quad . \quad . \quad \text{(iii)}$$

is termed the mean square contingency, and it clearly vanishes if the distribution of the two characters be independent.

$C_1 = \sqrt{\phi^2/(1+\phi^2)}$ is termed the coefficient of mean square contingency, and it measures the deviation of the two characters from independence. It approaches unity if the characters are causal, and is zero if they are independent.

The whole of the above brief résumé of the usual statistical methods of dealing with correlation is independent of any assump-

tion as to the distributions of frequency following the normal or Gaussian law of variation. Should they do so, however, we have

$$r = \eta = C_1 \quad . \quad . \quad . \quad . \quad \text{(iv)}$$

or all our three methods of determining the intensity of correlation merge theoretically into a single value.

We say theoretically, because the truth of (iv) depends upon our replacing summation by integrals, or it is the limit with sufficiently fine grouping. By actually testing (iv) on fairly Gaussian material it will be found that even moderately coarse classifications give us quite close results. Deviations from (iv) arise, not wholly, but chiefly through the characters dealt with being non-Gaussian in distribution of variability. We venture to think that possibly astronomers have been too ready to assume that all types of variability follow the Gaussian law of distribution, and that the assumption that star characters follow it requires statistical justification. It is, perhaps, rather dangerous to start with the doctrine that they must, and then deduce rather sweeping conclusions from the fact that they do not. In the present investigation the correlation coefficient, the correlation ratio, and the coefficient of mean square contingency will be used according to the nature of the statistics with which we have to deal.

The probable error of r is calculated from

$$p.e \text{ of } r = \cdot 67449(1 - r^2)/\sqrt{N}$$

and of η from

$$p.e \text{ of } \eta = \cdot 67449(1 - \eta^2)/\sqrt{N}.$$

These values are not the absolutely correct values, but are sufficiently close for most practical purposes.*

The probable error of C_1 is troublesome to calculate,† but if C_1 be two to three times greater than $\cdot 67449/\sqrt{N_1}$, this being the maximum value of the probable error, *i.e.* that when $C_1 = 0$, it will undoubtedly be significant.

(3) *Correlations with Stellar Colour.*—(i) *Colour and Magnitude.*—In the earlier memoir stellar colour and magnitude were correlated, using for this purpose the catalogue of star colours contained in vol. ix. of the *Annals of the Cape Observatory*. The stars included in that list range in magnitude from 4 to 10, being 4 to 9 for visual and 6 to 10 for photographic magnitude. The colours ranged from yellow to red, and contained no blue or green or white. In order to include green and blue stars we used, at the suggestion of Mr. T. W. Backhouse, the star catalogue in vol. xiv. of the *Harvard Annals*. That list includes stars of photographic magnitude $-1\cdot5$ to 7, and contains about 3·6 per cent. of blue and green stars. A table of contingency for magnitude and colour in

* For probable errors of r and η, see Pearson, "On the General Theory of Skew Correlation," pp. 19 and 20, *Drapers' Research Memoirs*, Dulau & Co.

† Blakeman and Pearson, "On the Probable Error of Mean Square Contingency," *Biometrika*, vol. v. pp. 191 *et seq.*

the case of 2834 stars is given below (Table I.). In grouping for contingency we reduced this to a 6×9-fold table, but the means were calculated on the basis of Table I. The results reached were as follows:—

	Previous Table. (*Monthly Notices*, vol. lxvi., p. 455.)	Present Table.
Mean magnitude . . .	8·58 ± ·05	5·15 ± ·012
Standard deviation . .	·835 ± ·032	·906 ± ·008
Coefficient of contingency	·304 (± ·05)	·271 (± ·01)

Thus while the two groups of stars differ very widely in mean character, we see that the dependence of one character on the other is essentially the same in the two samples. The reduction of the

TABLE I.

*Contingency Table for Colour and Photographic Magnitude.**

	Colour.						
Magnitude.	Blue.	Green.	White.	Yellow.	Orange.	Red.	Totals.
−1·5 to −1	...	...	1	...	...	...	1
−1 to −·5	...	...	...	...	...	...	0
−·5 to 0	...	...	...	...	...	...	0
0 to·5	2	...	1	1	...	...	4
·5 to 1	...	...	1	...	1	...	2
1 to 1·5	...	...	3	2	2	...	7
1·5 to 2	...	...	6	...	...	...	6
2 to 2·5	...	1	11	12	1	...	25
2·5 to 3	...	6	9	12	3	1	31
3 to 3·5	1	5	16	39	8	...	69
3·5 to 4	...	9	32	68	12	...	121
4 to 4·5	1	17	61	121	27	...	227
4·5 to 5	4	15	112	191	54	8	384
5 to 5·5	8	16	208	371	111	9	723
5·5 to 6	4	6	239	425	140	13	827
6 to 6·5	5	1	82	192	77	5	362
6·5 to 7	...	...	10	24	9	1	44
7 to 7·5	1	...	...	...	...	...	1
Totals,	26	76	792	1458	445	37	2834

* In the magnitude class, the group includes the first and excludes the second value given. Thus all stars being enumerated to one decimal in magnitude, the group 4 to 4·5 contains 4·0, 4·1, 4·2, 4·3 and 4·4 stars or centres at 4·2. The blank lines indicate the magnitude groups actually used in deducing the contingency coefficient from a 6×9 fold table.

mean magnitude by three classes, the introduction of blue, green, and white stars, has not essentially altered the numerical relationship between magnitude and colour. There are several points about this which deserve fuller consideration. Magnitude is often taken as a true variable. When dealing with the relationship of magnitude to parallax in the former paper it was shown that magnitude *used as a numerical variate*, unless we convert it into light units, does not give very satisfactory results, and that, further, the curves of means such as we find for stellar characters are generally widely different from straight lines. In the present case we cannot calculate the correlation coefficient r, because there is no quantitative scale of colour; we can, however, find the correlation ratio η on the assumption that magnitude is a true scalar quantity, which, of course, it is not. We find numerically $\eta = \cdot 17 \pm \cdot 01$. The results given on p. 456 of the former paper (for visual magnitude) lead to $\eta = \cdot 13 \pm \cdot 05$. Thus, again, the two results are alike within the range of probable error. But we note at once that if magnitude be used as a true quantitative character we shall not get results for C_1 and η which are really comparable. Indeed, a little thought will show that a mean magnitude is something having little physical meaning, and the ratio of two mean magnitudes, which is the nature of η, may often be deceptive.* We must accordingly anticipate erratic results when magnitude is used as a numerical variate, and we shall endeavour to adopt it only as an index to classification.

Breaking, however, for a moment through this good rule we may note that the mean magnitudes of the colour arrays are—

Colour.	Mag.	No.	Colour.	Mag.	No.
Red	5·38	37	Green	4·38	76
Orange	5·31	445	Blue	4·97	26
Yellow	5·18	1458	White	5·09	792

Whole series: Mag. 5·15 No. 2834

We see at once from this result that, within the limits of probable error, the mean magnitude of the white stars is the mean magnitude of the whole group of stars, or this group may be omitted when we determine the influence of deviation from mean colour on deviation from mean magnitude. The presence or absence of white stars does not affect the contingency. In the accompanying Diagram I.† a graph has been drawn of the variation of mean magnitude with

* The mean magnitude of two stars of magnitudes 5 and 10 is 7·5, but the magnitude of the star which has their mean amount of light is 5·74. The mean magnitude of two stars of magnitude 14 and 1 is also 7 5, but the magnitude of the star with their mean amount of light is 1·75. The ratio of the means found in the first manner is unity, in the second manner more than 3. Yet we perpetually see mean magnitudes compared and the difference between them asserted to be of significance or non-significance for the character of stellar groups.

† This diagram was taken out of its place in this paper and published, without the data on which it is based, in a letter to *Nature*, Oct. 17, 1907.

colour. To obtain a reasonable scale of colour, we have used that of the solar spectrum, plotting first a curve of mean magnitude, and then replacing it by a curve of star luminosity, the scale of luminosity being that in which the unit of luminosity is that of a star of magnitude 10. We have naturally omitted the white stars. (If the white stars be omitted from the Harvard data the value of the contingency is ·297, a value in absolute accordance with that found for the Cape stars in the earlier paper.) The graph shows at once a somewhat striking result,—the distribution of luminosity among the coloured stars gives a curve remarkably similar to that of luminosity in the solar spectrum if we shift the solar curve towards the violet end of the spectrum. How far is the method of determining magnitude influential in this matter? We have at present

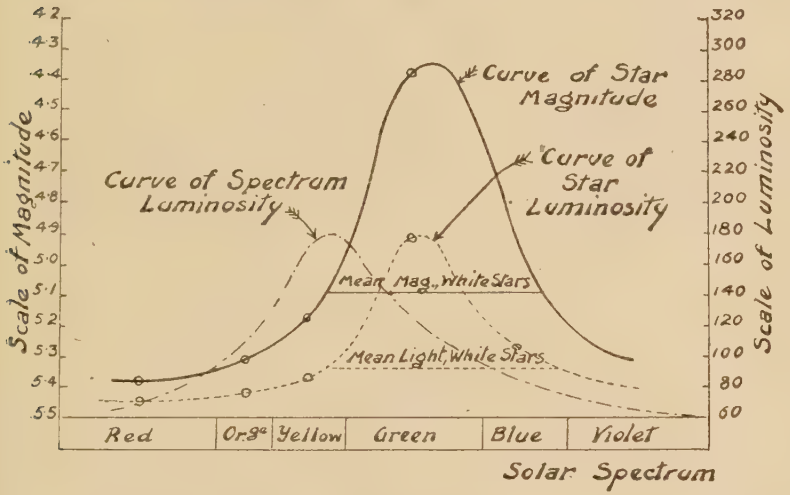


Diagram I.

in hand the Potsdam colour observations, and hope shortly to publish further results on this point.

(ii) *Colour and Spectral Class.* — As far as we are aware, no classifications of stellar colour according to parallax or proper motion have yet been made. Mr. Franks has recently published three tables * giving the classification of colour according to spectral class. These tables, as they stand, are actually contingency tables, but the smallness of some of the groups and the extremely laborious process of working out 7 × 12-fold contingency tables has led us to concentrate the material in rather larger colour groups, keeping, however, the spectral classes the same. We have taken as our colour groups (*a*) O, (*b*) YG^1, Y^1, O*r*Y^1 (practically the "white" group), (*c*) Y^2, (*d*) O*r*Y^2, O*r*^{2}, (*e*) Y^3, O*r*Y^3, O*r*^{3}, (*f*) O*r*R^3, R^3, thus making six colour groups or a 7 × 6-fold table. We owe to Dr. A.

* *Loc. cit.*, pp. 539–541.

Lee, of the Biometric Laboratory, University College, London, the calculation of the constants for Mr. Franks' three tables, with the following results:—

Correlation of Colour and Spectral Class.

432 Southern Stars from $-25°$ to S. Pole .	$C_1 = \cdot 71$
928 Northern Stars from $-25°$ to N. Pole .	$C_1 = \cdot 74$
1360 combined results	$C_1 = \cdot 71$

These results are of much interest and in very close accordance. There is no substantial difference between northern and southern stars in the relationship of colour and spectrum, but, as we might have anticipated, there is a very high relationship between the two characters. The relationship is more than double that which we have found between magnitude and colour. We shall see later that spectral class is far more clearly associated with magnitude than colour is.

Accordingly, we may sum up our first results on colour as follows:—The colour of a star depends to some extent on its magnitude, but in a far more marked manner on its spectral class.

It seems unnecessary to reproduce Mr. Franks' southern and northern star tables. His total star table is given here, with the groupings adopted, and in brackets the excess or defect of each group from the independent probability expectation.

TABLE II.

Contingency of Colour and Spectral Class.

Type.	O.	YG^1, Y^1, OrY^1.	Y^2.	OrY^2, Or^2.	Y^3, OrY^3, Or^3.	OrR^3, R^3.	Totals.
Helium stars	125	146	8	3	0	0	282
	(+63·62)	(+42·95)	(−36·37)	(−34·74)	(−33·18)	(−2·28)	
Hydrogen stars	168	195	14	0	0	0	377
	(+85·95)	(+57·23)	(−45·32)	(−50·45)	(−44·35)	(−3·05)	
α Carinæ	3	97	23	8	6	0	137
	(−26·82)	(+46·93)	(+1·44)	(−10·33)	(−10·12)	(−1·11)	
Solar stars	0	41	77	33	29	0	180
	(−39·18)	(−24·78)	(+48·68)	(+8·91)	(+7·82)	(−1·46)	
Arcturus	0	15	86	77	63	0	241
	(−52·45)	(−73·07)	(+48·08)	(+44·75)	(+34·65)	(−1·95)	
Aldebaran	0	0	4	22	43	6	75
	(−16·32)	(−27·41)	(−7·80)	(+11·96)	(+34·18)	(+5·39)	
Betelgeuse	0	3	2	39	19	5	68
	(−14·80)	(−21·85)	(−8·70)	(+29·9)	(+11·00)	(+4·45)	
Totals	296	497	214	182	160	11	1360

The great regularity of the differences certainly speaks for the excellence of the spectral and colour classes adopted by Mr. Franks.

(4) *Correlations with Spectral Class.*—We have seen in the pre-

ceding section that the association of spectral class and colour is markedly high, giving the mean square contingency coefficient $C_1 = \cdot 71$. Data for the correlations of spectral class with magnitude, proper motion, and parallax are given in the Yale Observatory memoir already cited.* It is true that the number of stars is small, but the total of 98 is larger than we were able to deal with when considering parallax in the first memoir. Further, some of the spectral classes have so small a frequency that, for statistical purposes, it was absolutely needful to group certain classes together. The classes actually used in the notation of the Draper Catalogue were—

Class A	=	13 Stars.
Class E	=	12 ,,
Classes F and G	=	34 ,,
Classes H and I	=	29 ,,
Class K	=	5 ,,
Classes M and Q	=	5 ,,

Practically, no weight can be placed on the last two results. Now parallax and proper motion are true quantitative characters, and η is accordingly the proper constant to calculate in these cases. It has also been found for magnitude. We again owe the whole numerical work to Dr. Lee. The results come out as follows :—

Correlation Ratio of Spectral Class and Parallax,	$\eta = \cdot 36 \pm \cdot 06$
,, ,, ,, Proper Motion,	$\eta = \cdot 39 \pm \cdot 06$
,, ,, ,, Magnitude	$\eta = \cdot 68 \pm \cdot 04$

All these results are significant compared with their probable errors, but before we discuss them it is well to ascertain whether the last result will be confirmed if we drop the idea of magnitude as a scalar quantity and proceed only by using it as an index to classification. Table III. is a contingency table for magnitude and spectral class for these 98 stars :—

TABLE III.

Contingency : Spectral Class and Magnitude Class. Draper Catalogue.

Magnitude.	A.	E.	F and G.	H and I.	K, M, and Q.	Totals.
− ·5 to + ·5	1	...	1	...	1	3
+ ·5 to 1·5	2	...	1	...	3	6
1·5 to 2·5	3	...	...	...	...	3
2·5 to 3·5	1	...	2	...	4	7
3·5 to 4·5	...	...	8	2	2	12
4·5 to 5·5	...	1	13	2	...	16
5·5 to 6·5	1	5	9	12	...	27
6·5 to 8	5	6	...	13	...	24
Totals	13	12	34	29	10	98

* *Loc. cit.*, see Table v. p. 204.

In working this table we have grouped in magnitude $-\cdot5$ to $1\cdot5$ and $1\cdot5$ to $3\cdot5$, or dealt only with a 6×5-fold table. The material is too sparse for finer classification. This table gives us $C_1=\cdot69(\pm\cdot07)$. It accordingly fully confirms the value obtained by the η method. It is, perhaps, worth noticing that it is in the first place the K, M, Q group, and then in the next place the F, G group, which contribute most to the value of the mean square contingency, *i.e.* these are the classes which have most markedly differentiated magnitudes. We see that the relationship between parallax and spectral class is only a little more than half that between magnitude and spectral class. Or we conclude that, on the basis of the Yale stars, the present chemical constitution of a star is of considerably greater importance than its distance in determining its magnitude.

It must, however, be remembered that 98 is rather a limited number to base definite statements upon; and as the point is of considerable interest, it is important to consider the matter from other aspects. Before the Yale memoir came to hand we had already formed Table IV., giving the magnitudes and spectral classes of over a thousand stars. It is based on the data provided in vol. xxviii., *Harvard Observatory Annals.* Pickering's 22 spectral classes would give a contingency table of unworkable magnitude, and accordingly we have grouped his spectral classes as follows, making a 7×8-fold table:—

TABLE IV.

Contingency of Magnitude and Spectral Class.

	Spectral Class.							
Magnitude.*	X.	Y.	Z.	V.	U.	W.	S.	Totals.
Below 1	4	1	2	4	1	1	...	13
1 to 2	6	1	10	5	2	1	1	26
2 to 3	9	...	22	15	3	1	2	52
3 to 4	13	12	34	62	4	9	...	134
4 to 5	70	31	87	153	25	20	...	386
5 to 6	50	24	60	69	5	11	2	221
6 to 7	58	22	51	40	6	5	6	188
7 and above	23	5	40	18	2	1	5	94
Totals	233	96	306	366	48	49	16	1114

Our Z = Pickering's groups 1 to 5; Y = 6; X = 7 to 11; V = 13 to 16; U = 17 to 20; W = 12; T = 21 (does not occur); S = 22.

The mean magnitudes determined from a $\cdot5$ grouping of the classes arranged in order of magnitude are as follows:—

* Magnitude 4 to 5, for example, contains all stars of 4th and less than 5th magnitude.

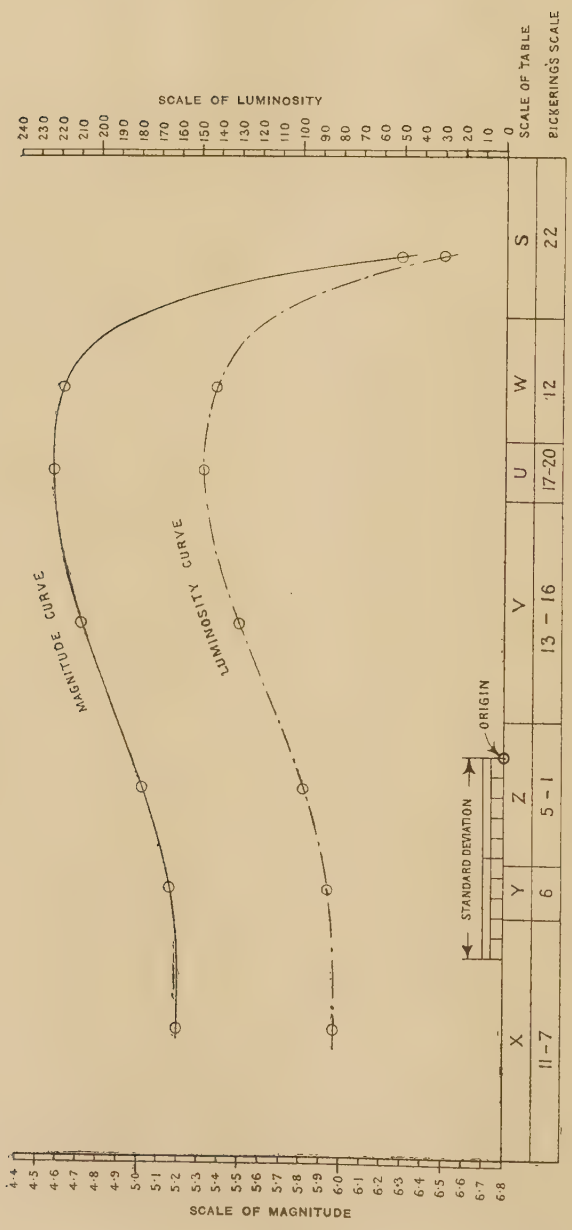
	Mean Magnitude.	No. of Stars.
Class S	6·29	16
„ X	5·20	233
„ Y	5·16	96
„ Z	5·02	306
„ V	4·71	366
„ W	4·62	49
„ U	4·57	48
Whole series	4·95 ± ·03	1114

Standard deviation of whole series = 1·49 ± ·02

The coefficient of mean square contingency for this long series was $C_1 = \cdot 43\ (\pm \cdot 02)$. Thus, while it is not as large as that found for the Draper Catalogue stars given in the Yale memoir, it still points to a fairly close relationship between spectral class and magnitude. It will, of course, be obvious that the order of mean magnitude of the spectral classes given above is not that of Pickering's class numbers, so that it may be presumed that the characters which led to his order of classification are not those which lead to order of magnitude. If we attempt to place our magnitudes in a continuous curve, we note that (i) more than one pair might be interchanged without affecting the limits fixed by the probable errors of random sampling, and (ii) we have no spectral scale upon which to plot the magnitudes or luminosities. The latter difficulty may be overcome in a manner which has proved of some service in other branches of statistical inquiry, namely, by assuming that the total spectral frequencies of each class are portions of a normal curve of frequency, and then plotting on verticals through the means (as deduced from a well-known property of the Gaussian curve) of the corresponding areas. Assuming that the groups, being merely qualitative, may be interchanged in order, it rarely happens that more than one arrangement gives a continuous curve. We have found Diagram II. by this method, plotting both magnitude and luminosity * curves. It will be seen that fairly smooth curves are reached in this manner; they would probably have been better had the U stars had greater frequency, and the V stars been dealt with in smaller groups. The diagram suggests, without of course proving, the likelihood of an ascent to a maximum luminosity from two different ends of a spectral classification.

It appeared probable that a classification of spectral characters based more admittedly on temperature considerations than those of either Secchi or Pickering would lead to a still high correlationship between magnitude and spectral character. Accordingly, appeal was made to Sir Norman Lockyer, who, with great kindness, provided a key index linking Pickering's classes with his own temperature classification. On further consideration, however, it

* The luminosity of the star of the 10th magnitude is taken as the unit and Pogson's value of the constant adopted.



Curves of Magnitude and Luminosity plotted to means for a normal frequency scale.

DIAGRAM II.

seemed probable that somewhat better results would be reached by working with Sir Norman Lockyer's own classified catalogue,* although this involved supplying from other sources (Harvard Photometry, where possible) the magnitudes of the recorded stars. We have from this catalogue formed Table V., giving spectral class and magnitude of 448 stars.

TABLE V.

Contingency: Stellar Magnitude and Spectral Class (Lockyer's Classification).

	Magnitude.							
Class.	Under 1·5.	1·5–2·5.	2·5–3·5.	3·5–4·5.	4·5–5·5.	5·5 and over.	Totals.	Means.
Argonian	...	1	1	...	...	...	2	...
Alnitamian	...	4	1	2	1	...	8	(2·95?)
Crucian	5	8	31	6	2	...	52	2·78
Achernian	1	1	1	8	7	...	18	4·00
Taurian	1	1	3	...	...	...	5	(2·35?)
Algolian	1	1	12	15	1	7	37	3·90
Rigelian	1	1	...	1	...	...	3	...
Markabian	...	4	8	5	1	...	18	3·12
Cygnian	1	...	1	...	...	...	2	...
Sirian	3	11	20	37	24	8	103	3·84
Polarian	...	5	2	6	...	...	13	3·02
Procyonian	2	3	10	20	5	1	41	3·55
Aldebarian	...	8	27	16	1	...	52	3·14
Arcturian	4	7	22	34	9	...	76	3·40
Antarian	2	4	7	3	1	...	17	2·71
Piscian	...	...	...	...	1	...	1	...
Totals	21	59	146	153	53	16	448	3·39

Here 1·5–2·5 signifies 1·5 and under 2·5. In finding the means, the group "under 1·5" was broken up into three groups.

The mean magnitude of the stars in this list was $3{\cdot}39 \pm 0{\cdot}4$, and their standard deviation $1{\cdot}158 \pm {\cdot}026$. They are thus much brighter and less variable than the stars of the two earlier series. The contingency coefficient for this grouping is

$$C_1 = {\cdot}54\,(\pm {\cdot}03).$$

We see accordingly that the association of magnitude and spectral class has been sensibly increased by using Sir Norman Lockyer's classification. The correlation is now about half way between that provided by Pickering's date and the 98 Yale stars.

* *Catalogue of 470 of the brighter Stars classified according to their Chemistry at the Solar Physics Observatory, South Kensington.*

Looking at the column of means, and rearranging it on Lockyer's plan,

Argonian ?

Alnitamian 2·95 ?

2·78	Crucian	Ascending Temperature.	Descending Temperature.	Achernian	4·00
2·35 ?	Taurian			Algolian	3·90
?	Rigelian			Markabian	3·12
?	Cygnian			Sirian	3·84
3·02	Polarian			Procyonian	3·55
3·14	Aldebarian			Arcturian	3·40
2·71	Antarian			...	
	...			Piscian	?

we see that the average magnitudes are greater on the side of "Descending Temperature," and less on the side of "Ascending Temperature," but the order of magnitude cannot be said to fit closely the classification order. The order is chaotic on the "Ascending Temperature" side; and although, with the exception of the Markabian class, it is orderly on the "Descending Temperature" side, the justification for the order does not lie in magnitude. When more material is available, it would seem that a spectral classification according to mean magnitude might be suggestive. All that we can venture to say at present is that magnitude is very far from being independent of spectral class, but that the reason why certain spectral classes have differentiated magnitudes is not evident in any of the classifications here dealt with.

(iii) *Spectral Class with Proper Motion and Parallax.*—We now turn to the relationship between spectral class and proper motion. In this case we have only dealt at present with the table provided in the Yale memoir, but there exists plenty of further data which we hope to work up. The correlation ratio for spectral class and the proper motions of the 98 stars given in that case is

$$\eta = \cdot 39 \pm \cdot 06.$$

Spectral class is thus shown to be definitely associated with proper motion. The association is only about half that of colour, and sensibly less than that of magnitude, still it is quite a considerable relationship. We might, *a priori*, anticipate that the association was an indirect effect of the correlation of spectral class and parallax, but it is easy to prove that this is at least only partly the case. The correlation ratio between parallax and spectral class deduced from the same material is

$$= \cdot 36 \pm .06.$$

This, again, is quite a considerable amount of relationship, but it is not greater but practically equal to the relationship of spectral class and proper motion.

Now if we have three characters, 1, 2, and 3, and if r_{12} be the correlation of the first and second without regard to the third, then

$$\rho_{12} = \frac{r_{12} - r_{13}r_{23}}{\sqrt{1 - r_{13}^2}\sqrt{1 - r_{23}^2}}$$

is the "partial" correlation coefficient of the first and second characters for a constant value of the third. In other words, if the relationship between 1 and 2 be solely due to the relationship of both to 3, we should expect that within the array of 1 and 2 corresponding to a constant value of 3, the correlation would be zero between 1 and 2, or $\rho_{12} = 0$. This means that $r_{12} = r_{13} \times r_{23}$ is the test for the correlation of 1 and 2 being solely due to their relationship to 3. Applied to our particular case, if the correlation between spectral class and proper motion be solely due to the relation between both and parallax, we should expect the correlation coefficient of spectral class and proper motion to be equal to the product of the coefficients of parallax with spectral class and parallax with proper motion. Now we have seen that the relationship of spectral class with proper motion and parallax is about equal. Accordingly, we should expect a relationship between proper motion and parallax not very far from perfect, or the correlation coefficient about unity. This is very far indeed from the case; it does not exceed ·3 to ·5. It follows, accordingly, that the relationship of spectral class to proper motion is not an indirect effect of chemical constitution being a function of spatial distribution. It is, of course, partly due to this result, but, to judge from the Yale data, there is a sensible relationship between chemical constitution and intensity of stellar motion in space.

We may form some notion of the amount of this, as follows. Judging from the values of the correlation ratios, we may assume the correlation coefficients to be not very different from ·4 for spectral class with proper motion or with parallax. The correlation between proper motion and parallax is also not far from ·4 (see § 6). Hence we find for the value of the partial correlation of spectral class and proper motion for constant parallax—

$$\rho_{12} = \frac{\cdot 4 - \cdot 4 \times \cdot 4}{\sqrt{1 - (\cdot 4)^2}\sqrt{1 - (\cdot 4)^2}} = \cdot 2857 = \cdot 3, \text{ say.}$$

Thus, in rough numbers, on the data at present available, about 25 p.c. only of the relationship between spectral class and proper motion is due to parallax. It is desirable to place in a table (Table VI.) the mean magnitudes, proper motions, and parallaxes, as given by the Yale results grouped as dealt with here. It is perhaps unnecessary to observe that far more extended frequencies are needful before the numbers given can be finally accepted.

While scarcely any weight at all is to be placed on the results for classes K, M and Q, we still see that for the remainder, while the orders of spectral class for parallax and proper motion are more alike than they are in either case to that for magnitude, they still differ considerably from each other.

TABLE VI.

Characters and Spectral Class.

Magnitude.		Parallax.		Proper Motion.	
Mean.	Spectral Class.	Mean.	Spectral Class.	Mean.	Spectral Class.
6·35	E	·084	F and G	·878	K
6·30	H and I	·068	E	·708	E
4·59	F and G	·065	A	·665	F and G
3·95	A	·054	K	·646	H and I
2·54	M and Q	·025	H and I	·421	A
1·88	K	·020	M and Q	·134	M and Q
4·98	All classes.	·057	All classes.	·616	All classes.

To sum up our results for spectral class we may conclude as highly probable—

That there is a very considerable relationship between magnitude and spectral classes, which will probably be found to be intensified if more satisfactory spectral categories can be determined. This relationship, however, is at present sensibly lower than the relationship between colour and existing spectral classes, which is very high. There are, further, fairly considerable relations, in part independent, between spectral class and both parallax and proper motion. It is thus probable that the chemical constitution of a star is not only associated with a certain colour and brilliancy, but also with its spatial position (as determined by its distance from us), its motion in space, and its size (so far as size as well as intensity is an attribute of magnitude).

(5) *Correlations with Magnitude.* — In the present paper we have already seen that magnitude is related to both colour and spectral class. We may note here that the relation of magnitude to spectral class is not due to the relation of both to colour. On the other hand, the relationships between spectral class and both colour and magnitude are so high compared with that between magnitude and colour, that it is highly probable that the partial correlation between colour and magnitude for a given spectral class is *negative*. In other words, if the colour classes were arranged in order of the average magnitude of the stars contained in them, then, if we confined our attention to one spectral class only, say the Arcturus type, the order of magnitude for the colour classes within this spectral class would be *reversed*.

(i) *Magnitude and Parallax.*—In the former paper this relationship was deduced from 72 stars given for parallax by Newcomb. The deficiency of material was fully admitted, and the results as to correlation could only be considered as preliminary. They indicated, however, (i) that we should not expect to find a very high association between magnitude and parallax; (ii) that the correlation coefficient differing widely from both the correlation ratio and the contingency coefficient, the distribution was far from

normal; and (iii) the line of means was not approximately straight, but the parallax rose again with the fainter stars. The Yale memoir provides far better material of more than double the amount, namely, 173 stars. It is desirable to discuss how far this diverges from the previous results. The Yale authors have grouped their arrays of parallax in somewhat irregular magnitude classes and get the following results (Table ii. p. 201, *loc. cit.*):—

TABLE VII.

No of Stars.	Range of Magnitude.	Mean Magnitude.	Mean Parallax.
10	0·0–1·5	0·81	″·0950
29	2·0–4·9	3·75*	·0666†
33	5·0–6·2	5·65	·0558
34	6·3–7·0	6·68	·0447
31	7·1–7·9	7·57	·0168
36	8·0–9·0	8·33	·0472

The mean magnitude for the whole series is $6{\cdot}16 \pm {\cdot}11$, with a standard deviation of $2{\cdot}06 \pm {\cdot}07$. The mean magnitude of the Newcomb series was $4{\cdot}03 \pm {\cdot}22$, with a standard deviation of $2{\cdot}74 \pm {\cdot}15$. It contained on an average much brighter stars, but was a more variable sample in brightness. It will be seen that, as in the first sample, parallax tends to increase again with the faintest stars. Working out η for the arrays of parallax corresponding to the above magnitude ranges, we have

$$\eta = {\cdot}28 \pm {\cdot}05.$$

The relationship, therefore, of magnitude to parallax is about equal to its relation to colour, and not more than half its relation to spectral class.

We can, however, approach the subject from another standpoint: we may inquire into the distribution of magnitude for given arrays of parallax. We have the following table deduced from our author's Table iii. (p. 202):—

TABLE VIII.

No. of Stars.	Range of Parallax.	Mean Parallax.	Mean Magnitude
7	−″·13 to −″·07	−″·110	7·34
29	−0·06 to −0·00	−0·025	6·36
66	+0·00 to +0·06	+0·031	6·76
44	+0·07 to +0·13	+0·097	6·10
17	+0·14 to +0·20	+0·159	6·34

The mean parallax of the whole series is $0''{\cdot}0460 \pm ''{\cdot}0035$, with a standard deviation of $0''{\cdot}0664 \pm ''{\cdot}0025$. In the earlier series the mean parallax was $0''{\cdot}145 \pm ''{\cdot}011$, with a standard deviation of $0''{\cdot}134 \pm {\cdot}007$, indicating that we were dealing, on the whole, with a nearer set of stars, but with a greater variability in distance. It is obvious that very little can be judged from the mean magni-

* 3·748; the authors have 3·8. † The authors have ″·066.

tudes, especially if we exclude the first two categories of negative parallaxes.* We have included them because the authors do, but it is not easy to see why the 7 stars with the greatest negative parallaxes are to be looked upon as a class with a really lower quantitative parallax than those of the 29 group with a less negative parallax. Since the parallax cannot be below zero, what the results signify is that an average error of 0″·11 was possible in the group of 7, and one of 0″·025, or about a quarter of it, possible in the group of 29 stars. Errors of observation being thus more liable in the 7 group, it might even be reasonable to suppose members of this group to have, on the whole, higher true parallaxes than those of the 29 group; and the appearance of this group *at the top* with the high magnitude 7·34 is somewhat misleading. Leaving it, however, to form part of the series, we find

$$\eta = \cdot 20 \pm \cdot 05,$$

or we conclude that the determination of magnitude from parallax is considerably more inaccurate than that of parallax from magnitude. The association has indeed fallen below anything yet dealt with in the present paper. The inequality of the two values of η shows that our distribution is very far from Gaussian. It is clear that in these circumstances contingency is the only method by which we can approach a *unique* measure of the relationship of parallax and magnitude, and accordingly the following contingency table has been prepared connecting the two characters. It gives us

$$C_1 = \cdot 32 \ (\pm \cdot 05).$$

In the previous memoir for Newcomb's 72 stars we had

$$C_1 = \cdot 41 \ (\pm \cdot 08).$$

TABLE IX.

Contingency: Magnitude and Parallax.

Magnitude.	Parallax.					Totals.
	−0″·13 to −0″·07.	−0″·06 to 0″·00.	0″·00 to 0″·06.	0″·07 to 0″·13.	0″·14 to 0″·20.	
Below 3·95†	...	4	3	7	2	16
3·95 to 4·95	...	1	5	6	1	13
4·95 to 5·95	...	4	8	4	4	20
5·95 to 6·95	2	9	13	8	3	35
6·95 to 7·95	3	6	21	11	2	43
Above 7·95‡	2	5	16	8	5	36
Totals	7	29	66	44	17	163

* We would place considerable stress on this, because much of the impressiveness of the above table, both visually and when reduced numerically, lies on the place assigned to the group of largest negative parallaxes, with their mean magnitude 7·34. Now this consists of only 7 stars, and the mean magnitude may be merely indicative of greater errors arising with fainter stars.

† With the exception of three stars, 2·3 2·0, and 2·8, all were above 3·0.

‡ All below 9, with the exception of one star 9.

Thus practically the previous result and the present for the contingency agree within the limits of the probable error of the difference. Further, the contingency and correlation ratio results are also in close agreement, assuming we class by magnitude. Now the series we are considering at present is very different from the earlier series: that series contained 20 stars brighter than magnitude 2, while the present contains *none*. It contained only 7 stars above 7·95, while the present series contains 36. It is perfectly correct that in both cases stars have been frequently selected because they had large proper motions, and so may have large parallaxes. But does this really affect the argument that there is comparatively small relationship between magnitude and parallax? In order to do so, it must mean that the partial correlation is much lower than the absolute correlation, and this can only happen if the correlation between magnitude and proper motion is very high. To this point we now turn, because some criticism of the earlier paper has been made on this ground.

(ii) *Magnitude and Proper Motion.*—Any amount of material is forthcoming, of course, on this point, but it seemed advisable to work with the best available proper motions. Accordingly we used the *Catalogue of* 627 *Principal Standard Stars*, 1904, by Lewis Boss. Omitting variables, we had 303 stars with positive, 317 with negative, proper motions in R.A.* There were 195 stars with positive and 426 stars with negative proper motion in declination. There was no necessity to separate these groups, as we are dealing only with the size and not sign of the proper motions, but it was desirable to separate into random groups in order to test the steadiness of the coefficients, and the positive and negative groups served as well as any others.

Four tables of contingency were formed, given as X., XI., XII. and XIII. below. We owe the working of the contingency coefficients of the first two tables to Mr. A. M. Pritchard, of the Hartley University College, Southampton, and the last two to Dr. Alice Lee.

The statistical constants reached are as follows:—

Nature of Proper Motion.	Mean Magnitude.	Standard Deviation in Magnitude.	Contingency.
Positive in R.A.	3·63±·05	1·16±·03	·30 (±·04)
Negative in R.A.	3·79±·04	1·12±·03	·38 (±·04)
Positive in Declination	3·88±·06	1·16±·04	·36 (±·05)
Negative in Declination	3·72±·04	1·08±·02	·23 (±·03)

The mean degree of relationship between proper motion and magnitude is accordingly ·32.

In Diagrams III. and IV. the means of arrays of magnitude for given classes of proper motion are plotted; it will be seen at once how little influence of an orderly kind a selection of stars by proper motions would have on their magnitude.

* One star out of the 621 non-variables has been overlooked in tabling the proper motions in R.A. It lies in group of magnitude 3 5-4·4.

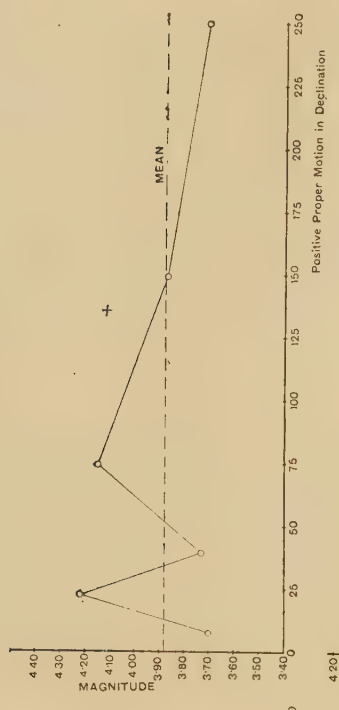


DIAGRAM IV.

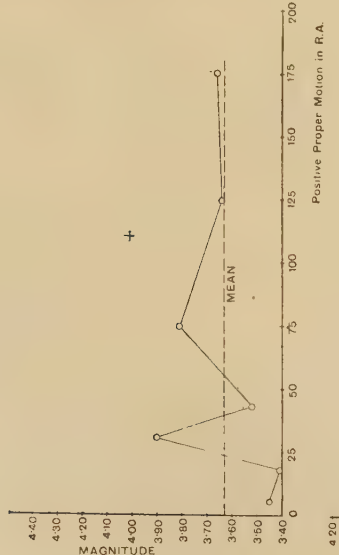


DIAGRAM III

Boss, of course, deals only with a selected group of stars, but we can test the result more or less closely on the Yale observations, which give total proper motions for magnitude classes, and magnitudes for total proper motion classes. We owe to Dr. Lee the correlation ratios for the two cases; they are respectively—

Correlation ratio: proper motions for magnitude classes, $\eta = \cdot 43 \pm \cdot 04$
„ „ magnitudes for proper motion classes, $\eta = \cdot 22 \pm \cdot 05$

The average relationship between the two characters is thus again about ·33. Thus we may safely conclude that the deviation

TABLE X.

Magnitude and Proper Motion in R.A. (μ positive).

	Magnitude.						
	Less than 1·5.	1·5 to 2·4.	2·5 to 3·4.	3·5 to 4·4.	4·5 to 5·4.	5·5 and over.	Totals.
0 to 11	2	9	19	21	11	2	64
12 to 24	2	3	8	23	3	...	39
25 to 36	...	2	5	12	9	...	28
37 to 49	1	3	7	11	3	1	26
50 to 99	1	4	8	31	12	2	58
100 to 149	1	7	8	15	8	3	42
150 and over	3	5	7	19	8	4	46
Totals	10	33	62	132	54	12	303

TABLE XI.

Magnitude and Proper Motion in R.A. (μ negative).

	Magnitude.						
	Less than 1·5.	1·5 to 2·4.	2·5 to 3·4.	3·5 to 4·4.	4·5 to 5·4.	5·5 and over.	Totals.
0 to 11	1	7	19	20	13	4	64
12 to 24	...	1	12	19	17	...	49
25 to 36	2	3	12	10	6	2	35
37 to 49	...	2	7	12	4	...	25
50 to 99	...	2	14	29	10	3	58
100 to 149	...	1	5	15	2	...	23
150 and over	3	6	11	21	13	9	63
Totals	6	22	80	126	65	18	317

TABLE XII.

Magnitude and Proper Motion in Declination (μ' positive).

Proper Motion.	Magnitude. Less than 1·5	1·5 to 2·4.	2·5 to 3·4.	3·5 to 4·4.	4·5 to 5·4.	5·5 and over.	Totals.
0 to 15	2	9	16	20	7	7	61
16 to 29	...	1	7	15	10	4	37
30 to 49	...	2	9	11	3	2	27
50 to 99	...	...	6	11	5	3	25
100 to 199	...	1	6	12	4	1	24
200 and over	2	...	4	11	3	1	21
Totals	4	13	48	80	32	18	195

TABLE XIII.

Magnitude and Proper Motion in Declination (μ' negative).

Proper Motion.	Magnitude. Less than 1·5.	1·5 to 2·4.	2·5 to 3·4.	3·5 to 4·4.	4·5 to 5·4.	5·5 and over.	Totals.
0 to 15	3	10	20	40	20	3	96
16 to 29	2	8	15	29	18	1	73
30 to 49	2	4	26	31	15	3	81
50 to 99	1	5	13	39	21	3	82
100 to 199	2	10	11	18	6	2	49
200 and over	2	5	9	22	7	...	45
Totals	12	42	94	179	87	12	426

from independent variation in the distribution of magnitude and proper motion is not very far from ·35. It is therefore possibly slightly higher than the value found for magnitude and parallax, which is about ·30. It will, we think, be evident that a selection by proper motions would have *comparatively* small effect in modifying the relation found between magnitude and parallax. Some rough estimate of the order of the change can be formed by considering what would be the absolute coefficient of correlation resulting from a partial coefficient of ·30 obtained by selecting stars of a single proper motion only; this will probably give an upper limit. We have

$$\rho_{12} = \frac{r_{12} - ·35 \times r_{23}}{\sqrt{1-(·35)^2}\sqrt{1-r_{23}^2}}$$

Here r_{23} is the correlation between proper motion and parallax. In the former paper for the 72 stars this was found to be about ·4 (p. 449); from the Yale data (see below) it is under ·38. Assuming it = ·38, and putting $\rho_{12} = ·30$, we deduce $r_{12} = ·39$, say. We think, therefore, we may safely assert that the relationship between parallax and magnitude, if no selection by proper motions had taken place, is not likely to prove as great as ·4, and probably lies considerably under this value.

We would suggest, therefore, that the lowness of the magnitude and parallax result obtained in the former paper is not due to any special selection of parallax stars by their proper motions. This could not largely influence the relationship of magnitude and parallax, because proper motion is only moderately correlated with parallax, and still less with magnitude.

(iii) Before we leave the subject of the correlation of magnitude and proper motion, it is worth while noting that it has also been investigated in an entirely different manner. In the above inquiry we have found the correlation ratio for the total proper motion and the magnitude in the case of the Yale stars; we have further calculated the contingency coefficients between two sets of groups of magnitude and proper motion in R.A. and declination respectively from the Boss Catalogue stars. But for other purposes the proper motion in declination, μ', and the proper motion perpendicular to a declination circle, $\mu \cos \delta$, had been found and taken out. Now we are concerned only with the size of the proper motions, and not their sense, at present. We divided, however, our data into two halves for the purpose of checking results. The stars from the equator to the North Pole, 298 in number,* have been taken in one group, and those from the equator to the South Pole, 323 in number, in a second.

Further, to avoid lengthy analysis, we dealt separately with proper motion parallel to the declination circle (μ') and perpendicular to it ($\mu \cos \delta$). Thus we have four series to discuss. The classifications of magnitude were as follows: under 1·5, 1·5 and under 2·5, 2·5 and under 3·5, . . . 6·5 and over. The proper motions are of such a wide range that no grouping was adopted, but the means and standard deviations found by the laborious processes of adding and of squaring. No stars were omitted, although one is sorely tempted to omit those two or three of abnormal proper motions, as undoubtedly abnormal on any probability test. Any selection of this or any other kind must, however, be for our present purpose dangerous, and accordingly we have allowed equal weight to all the available stars in Boss's list.†

The following results were obtained:—

* 6 variable stars in Boss's Catalogue were of necessity omitted.

† It may be of interest to note that, working by correlation methods on the Boss stars I find for the apex of the Sun's way R.A. = 275°, $\delta = +28°·5$, but the requisite conditions for a random distribution are hopelessly unfulfilled. The correlations directly provide a method of approaching the problem of multiple drifts which I hope to deal with later.—K. P.

TABLE XIV.

Northern Stars.

Magnitude.	No.	μ'.	$\mu \cos \delta$.
Under 1·5	9	372·44	214·33
1·5 to 2·4	25	71·08	80·00
2·5 to 3·4	61	71·15	64·59
3·5 to 4·4	137	103·35	76·85
4·5 to 5·4	54	93·70	94·81
5·5 to 6·4	9	66·22	85·00
6·5 and over	3	55·33	50·33
All stars	298	98·83	81·99

The complete constants for the whole of the 298 stars are—

Mean μ' $98{\cdot}83 \pm 7{\cdot}73$.
Standard deviation of μ' $196{\cdot}85 \pm 5{\cdot}47$.
Mean $\mu \cos \delta$ $81{\cdot}99 \pm 6{\cdot}66$.
Standard deviation of $\mu \cos \delta$ $170{\cdot}40 \pm 4{\cdot}71$.

TABLE XV.

Southern Stars.

Magnitude.	No.	μ'.	$\mu \cos \delta$.
Under 1·5	7	44·29	48·86
1·5 to 2·4	30	78·60	55·93
2·5 to 3·4	81	75·80	58·28
3·5 to 4·4	122	101·11	72·36
4·5 to 5·4	65	119·51	78·05
5·5 to 6·4	13	47·15	35·00
6·5 and over	5	22·40	17·20
All stars	323	91·76	65·58

The complete constants for the whole of the 323 stars are—

Mean μ' $91{\cdot}76 \pm 8{\cdot}87$.
Standard deviation of μ' $236{\cdot}33 \pm 6{\cdot}27$.
Mean $\mu \cos \delta$ $65{\cdot}58 \pm 6{\cdot}05$.
Standard deviation of $\mu \cos \delta$ $161{\cdot}31 \pm 4{\cdot}28$.

At first sight it might appear that the northern group of stars had a larger proper motion than the southern, but, considering the probable errors, it is doubtful whether any stress can be laid on this. Thus we have for southern and northern groups—

Difference of μ' . . . . . $= 7{\cdot}07 \pm 11{\cdot}77$
Difference of $\mu \cos \delta$ . . . $= 16{\cdot}41 \pm 9{\cdot}00$
Difference of S. D.'s of μ' . $= 38{\cdot}48 \pm 8{\cdot}32$
Difference of S. D's of $\mu \cos \delta$ $= 9{\cdot}18 \pm 6{\cdot}36$

Only in the third case is the difference more than twice its probable error, and it may just be that proper motions in declination in the southern hemisphere are more variable than in the northern, but much larger numbers would have to be used really to demonstrate this.

If, on the other hand, we compare the results for proper motion along and perpendicular to the declination circle, we have—

Northern Stars:

Difference of mean μ' and mean $\mu \cos \delta$ $= 16{\cdot}84 \pm 10{\cdot}20$

Difference of S. D. of μ' and S. D. of $\mu \cos \delta = 27{\cdot}36 \pm 7{\cdot}22$

Southern Stars:

Difference of mean μ' and mean $\mu \cos \delta$ $= 26{\cdot}18 \pm 10{\cdot}74$

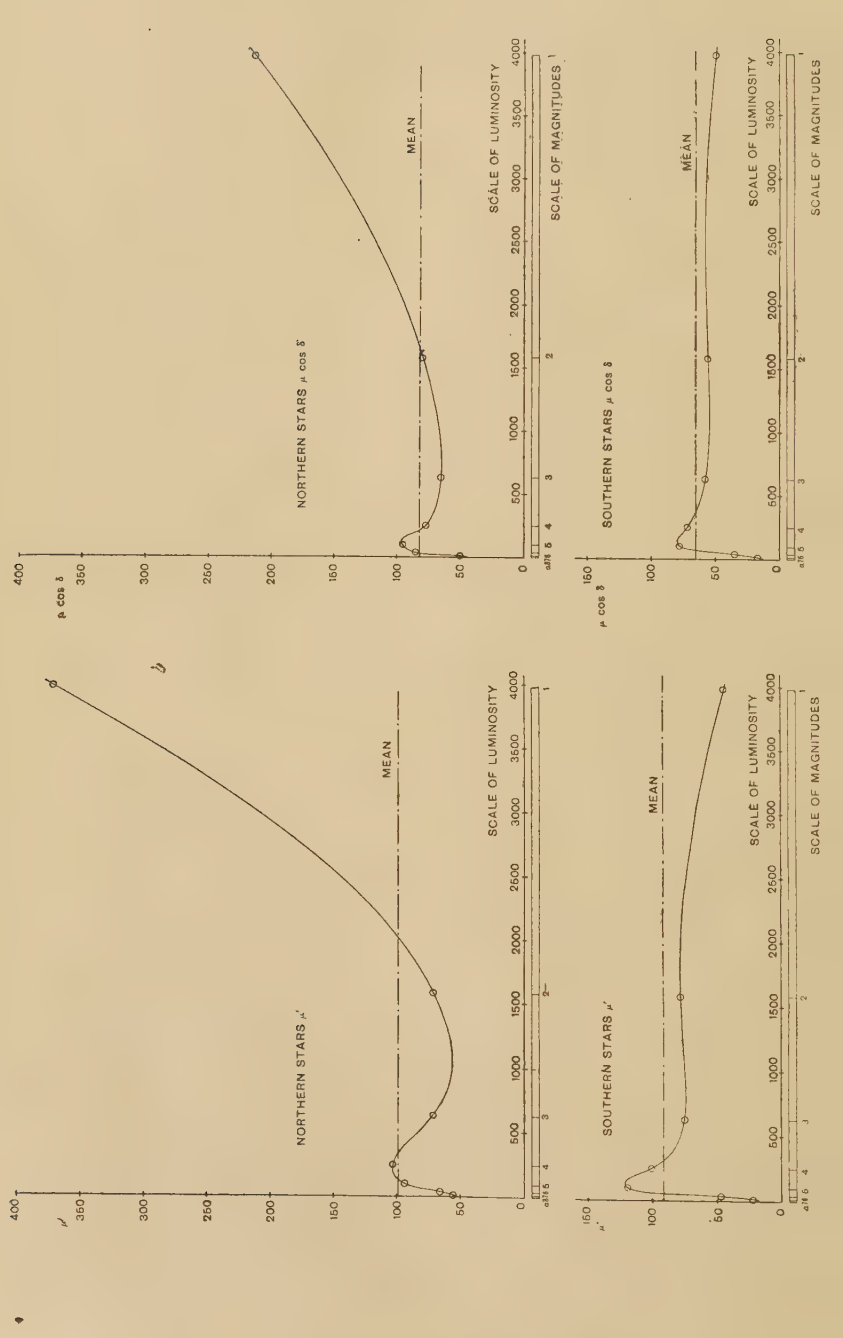
Difference of S. D. of μ' and S. D. of $\mu \cos \delta = 75{\cdot}02 \pm 7{\cdot}59$

Three of these differences are more than twice their probable errors; and looking at the values as a whole, it seems not improbable that proper motion in the declination circle is larger and more variable than proper motion perpendicular to it. The point deserves fuller consideration, especially in its relation to the position of the apex of the Sun's way. We shall return to the matter later, from another standpoint.

If we now examine the means of the magnitude arrays, we notice at once some very remarkable points. Regarding the mean values of both proper motion components, we note that, with one exception to be discussed below, the deviations from the mean values of the means of the arrays are small as compared to the corresponding variabilities, but these deviations are remarkably regular.

Taking the southern stars as typical, we find (Diagram V.) a continuous rise in proper motion for both components until stars of about 4·5 magnitude, then the proper motion falls again and becomes still smaller for the faintest stars. The same rule is apparent in the northern stars also, except that in the group of extremely bright stars we have a very large proper motion, which again, however, when we remember size of mean and S. D., is not so big as it appears. Still it is definitely significant, and marks the group of brightest northern stars as significantly different from the corresponding group of southern stars. The Diagram V. will bring out the remarkable character of these magnitude and proper motion curves.

If the determining link between magnitude and proper motion were parallax, surely we might anticipate a uniformly decreasing proper motion with increasing magnitude? Yet we see in all four cases the same phenomenon—the low value of proper motion for the stars of 1·5 to 3·5 magnitude, its growth to a maximum, and then its ultimate fall. It is true the variations are small, but their comparative regularity is very great. It is difficult to see what special selection of these standard stars could have led to this result. It is one that will be tested on much larger masses of material, but Boss's Catalogue was selected on account of the high


NORTHERN STARS μ cos δ
MEAN
SCALE OF LUMINOSITY
SCALE OF MAGNITUDES
SOUTHERN STARS μ cos δ
MEAN
SCALE OF LUMINOSITY
SCALE OF MAGNITUDES
NORTHERN STARS μ'
MEAN
SCALE OF LUMINOSITY
SCALE OF MAGNITUDES
SOUTHERN STARS μ'
MEAN
SCALE OF LUMINOSITY
SCALE OF MAGNITUDES

accuracy of his results and the manageable number of stars provided. We may look at the point from a second aspect. The lines of means are curved, and accordingly the coefficient of correlation is not the proper measure of the association between magnitude and proper motion. It is provided by the correlation ratio η. We have

Northern Stars:

Correlation ratio of magnitude and μ' $\eta = \cdot 26 \pm \cdot 04$

" " " $\mu \cos \delta$, $\eta = \cdot 15 \pm \cdot 04$

Southern Stars:

Correlation ratio of magnitude and μ' $\eta = \cdot 09 \pm \cdot 04$

" " " $\mu \cos \delta$, $\eta = \cdot 08 \pm \cdot 04$

It will thus be seen that the amount of correlation even for the northern stars is only small. Working on the 173 stars of the Yale memoir, we have found the correlation ratio for proper motion in arrays of limited magnitude; it is:

$$\eta = \cdot 22 \pm \cdot 05.$$

Considering that the Yale stars are all northern stars, but that in this case the *total* proper motion has been taken, and not the component proper motions, we see that the agreement, notwithstanding the wide difference of material, is excellent,—well within the limits of the probable errors.

While the correlation is slight for the northern stars, it is practically of little or no service, as far as selection goes, for the southern stars, or we must conclude that—

A selection of stars by their proper motions must have very little influence indeed on the relation between magnitude and parallax.

It will accordingly require some very stringent form of investigation to demonstrate that the form of the parallax-magnitude curve can be sensibly influenced by a proper motion selection.

As far as the present data extends, we would venture to suggest, therefore, that the criticism that the stars dealt with are specially selected is not really a valid one, for this selection would not largely influence the relationship of magnitude and parallax. We must conclude that the present results confirm the early ones, namely, they show that magnitude is a quantity much more closely associated with chemical condition (spectral class) than with distance; indeed, the association with colour is almost as great as the association with distance.

(6) *Correlations with Parallax.*—We have already dealt with the association between parallax and magnitude, colour and spectral class (pp. 422, 423, 428, and 432). The remaining feature is proper motion. This is provided by the Yale observations. Accepting the authors' proper motion classes, the arrays of parallax give us (Table I.),

Correlation ratio, parallaxes for proper motion classes,* $\eta = \cdot 36 \pm \cdot 05$

* This result is for total proper motion in a great circle, but whether we use component proper motions in arcs or again the values in R.A. and δ makes but little difference in the correlation values.

and again (Table III.),

Correlation ratio, proper motions for parallax classes, $\eta = \cdot 38 \pm \cdot 04$

The previous work on the 72 stars gave—

Correlation coefficient, proper motion in R.A. and parallax, $r = \cdot 44 \pm \cdot 06$
,, ,, ,, in declination and parallax, $r = \cdot 41 \pm \cdot 07$

Considering the size of the probable errors, the present results are reasonably in accordance with the former, or we may take it that the correlation between parallax and proper motion is not far from linear, and of magnitude about ·40. Thus we have the following scheme of relationship :—

Parallax and proper motion	·38
Parallax and spectral class	·36
Parallax and magnitude	·30

Thus, while the distance of a star is sensibly related to its proper motion, this relationship is not really more significant than the relation to spectral class ; and it is quite possible that if the spectra were assorted according to parallax, it would be possible to form spectral categories which would give a far higher association between parallax and spectral class than between the former and proper motion. The parallax stars have not yet been dealt with as to colour.

We have seen that, on the basis of the Yale stars, the correlation between parallax and proper motion is nearly linear. It is accordingly of considerable interest to obtain the line giving the mean parallax π_m for a group of stars with a given total proper motion p in a great circle. Let $\bar{\pi}$ be the mean parallax, $\bar{p}$ the mean proper motion, σ_π the standard deviation in parallax, σ_p the standard deviation in proper motion of the whole group of the Yale stars. Then we have—

$$\bar{\pi} = 0''\cdot 0460 \pm \cdot 0035, \qquad \sigma_\pi = 0''\cdot 0664 \pm \cdot 0025$$
$$\bar{p} = 0''\cdot 6763 \pm \cdot 0205, \qquad \sigma_p = 0''\cdot 3997 \pm \cdot 0205$$

we may take $r = \eta_{13} = \cdot 4006$.

Thus
$$\pi_m - \bar{\pi} = \frac{r\sigma_\pi}{\sigma_p}(p - \bar{p}),$$

or, in numbers,
$$\pi_m = 0''\cdot 0010 + 0\cdot 0666p,$$
$$= \tfrac{1}{1000} + \tfrac{1}{15}p.$$

If we neglect the $\frac{1}{1000}''$ as negligible in the case of parallax, we conclude that the mean parallax of an array of stars of given proper motion is one-fifteenth of that proper motion.* This is absolutely identical with the statement made by Newcomb, largely on theoretical grounds : †

* The 70 odd stars dealt with in the earlier memoir, if we compare total proper motion in a great circle and parallax, give the correlation of parallax and proper motion as ·58 and the regression coefficient $\frac{1}{17}$. This is as close to $\frac{1}{15}$ as we could expect from the material.

† *The Astronomical Journal*, vol. xxii. p. 169, 1902.

"That is, if we measure the parallaxes of all the stars having a given proper motion, we may expect the mean result to be about $\frac{1}{1\cdot5}$ of the proper motion."

The interesting point of this result is that the value which was given on theoretical grounds by a brilliant astronomer, and appears then to have passed unquestioned, should have met with disapproval when actually found from observations by the statistician. As we have just indicated, Newcomb's $\frac{1}{1\cdot5}$ is but little more or less than our statement that the correlation between proper motion and parallax equals ·4. Newcomb's

$$\text{mean } \pi = 0\cdot064\mu,$$

where μ = our p, is only a regression line without the constant term, which in this case, if it be not actually zero, is certainly very small. As far, then, as our reduction of the Yale data goes, it tends to confirm Newcomb's theory of stellar distribution; it also shows that our values of the parallax and proper motion correlation and of the parallax and proper motion standard deviations were reasonable values.

We find from the Yale data that the mean proper motion p_m for stars of a given parallax π is given by the regression line

$$p_m = 0\cdot5654 + 2\cdot4114\pi.$$

This does not agree with Newcomb's relation (*loc. cit.*, p. 168), and, of course, only applies to the range of stars in the Yale data. These, however, do not appear to satisfy Newcomb's equation.

(7) *Parallactic Motion.*—Still another method of approaching the parallax and proper motion correlation may be deduced from the Sun's motion. Let v = the velocity of the Sun, ρ the distance of a given star, and i = the index v/ρ. Then, if π be the parallax, p the proper motion of the star, and a the radius of the Earth's orbit,

$$\pi = \frac{a}{v} i,$$

where π and i may be measured in seconds of angle.

Since v/a is a constant, we have

$$r_{ip} = r_{\pi p},$$

or the correlation of parallax and proper motion is the same as the correlation of this index and proper motion. Suppose that σ_x represents the standard deviation of any variable x, then

$$\sigma_\pi = \sigma_i a/v,$$

and

$$r_{ip}\sigma_i/\sigma_p = \frac{v}{a} \times r_{\pi p}\sigma_\pi/\sigma_p.$$

Now $r_{ip}\sigma_i/\sigma_p = m$ is the slope of the best fitting line to the curve in which the mean value of i is plotted to a given proper motion class of stars.

The value of m has not been found at present because of the

labour of determining i for small groups of proper motions, but some idea of its value for large groupings of proper motions can be obtained from the papers by Oscar Stumpe* and by Messrs. Dyson and Thackeray.† These give us—

TABLE XVI.

Dyson and Thackeray.				*Stumpe.*			
No. of Stars.	Proper Motion. Group.	Presumed Mean.	Value of i.	Proper Motion. Group.	Actual Mean.	Value of i.	No. of Stars.
2885	0″·00 to 0″·05	0″·025	0″·017				
800	0″·05 to 0″·10	0″·075	0″·045				
316	0″·10 to 0″·20	0″·150	0″·084				
163	0″·20 and over	0″·312	0″·251	0″·16 to 0″·32	0″·23	0″·140	551
...	...	...	...	0″·32 to 0″·64	0″·43	0″·295	340
...	...	...	..	0″·64 to 1″·28	0″·85	0″·608	105
...	...	...	...	1″·28 and over	2″·39	2″·057	58

It will be seen that Stumpe worked with stars of far larger proper motion than Messrs. Dyson and Thackeray. We have only been able to give the presumed means in the case of the first set of stars as the actual values of the total proper motion in a great circle are not tabled. Assuming the means to approximate to the values given, we find for the first series

$$m = \cdot 7395$$

and for the second series

$$m = \cdot 8767$$

using the method of least squares to determine $m = r_{ip}\sigma_i/\sigma_p$,—*i.e.* the correlation process. It will be seen at once that the two sets of observations give no close agreement; the ranges of stars dealt with are very different.‡ If we take $m = \cdot 88$ we find

$$r_{\pi p}\frac{\sigma_\pi}{\sigma_p} = \cdot 88\frac{a}{v}.$$

* *Astronomische Nachrichten*, Bd. 125, ss. 385–426.

† *Monthly Notices*, vol. lxv. pp. 429.

‡ The graph of the Dyson-Thackeray and Stumpe data shows that their first three points form a more or less continuous curve with his four points, but that if our estimation of the mean proper motion of the stars above 0″·20 proper motion be at all correct, the fourth Dyson-Thackeray point does not lie on this continuous curve. This is possibly due to a considerable under-estimation of the mean value of the "over 0″·20" group. If we omitted this fourth point, we should get an initial slope m_0 to the regression curve of less than ·5, a value considerably nearer that required to reconcile the parallax and Sun's motion observations discussed below. The actual regression lines

$$i_m = \cdot 7395\, p - 0'' \cdot 004 \text{ (Dyson-Thackeray)}$$

$$i_m = \cdot 8767\, p - 0'' \cdot 074 \text{ (Stumpe)}$$

differ significantly. This is due to the fact that the regression curve is far from linear.

Now if v be 16 miles per second $v/a = 5{\cdot}4$ about, and accordingly

$$r_{\pi p}\frac{\sigma_\pi}{\sigma_p} = {\cdot}16 = \frac{1}{6} \text{ about.}$$

This is very different from the $\frac{1}{15}$ which has been given in the present memoir and by Newcomb. It may be said that this is due to the value found for the correlation of parallax and proper motion being too small. We can examine this as follows :—

Every correlation coefficient must be less than unity. Hence

$$r_{\pi p} = \frac{1}{6}\frac{\sigma_p}{\sigma_\pi},$$

with the above values, must be less than unity. Now the variability of proper motion is fairly well known. We find it to be $0''{\cdot}6$ for Stumpe's 1054 stars. It is $0''{\cdot}4$ for the Yale stars. Let us put it $0''{\cdot}5$, then

$$r_{\pi p} = \frac{1}{12\sigma_\pi},$$

and accordingly σ_π must be $> \frac{1}{12}$ or $> {\cdot}083$. The Yale results give $\sigma_\pi = {\cdot}066$. We thus reach an impossible value for the correlation of parallax and proper motion. We need σ_π to be about $0''{\cdot}2$ to get this correlation a reasonable value. The curious part about this result is this : If e be the error in determining the true parallax π', the observed parallax $\pi = \pi' \pm e$, and accordingly

$$\sigma_\pi{}^2 = \sigma_{\pi'}{}^2 + \sigma_e{}^2.$$

There cannot be a doubt that σ_e is large; thus we should expect the true value of variability in parallax to be considerably less than the observed value, owing to the liability of parallax to error.

The only suggestions that we note for overcoming the difficulty are—

(i) That a much higher value must be attributed to the velocity of the Sun than is usually adopted. Spectroscopic determinations, however, indicate a lower value.

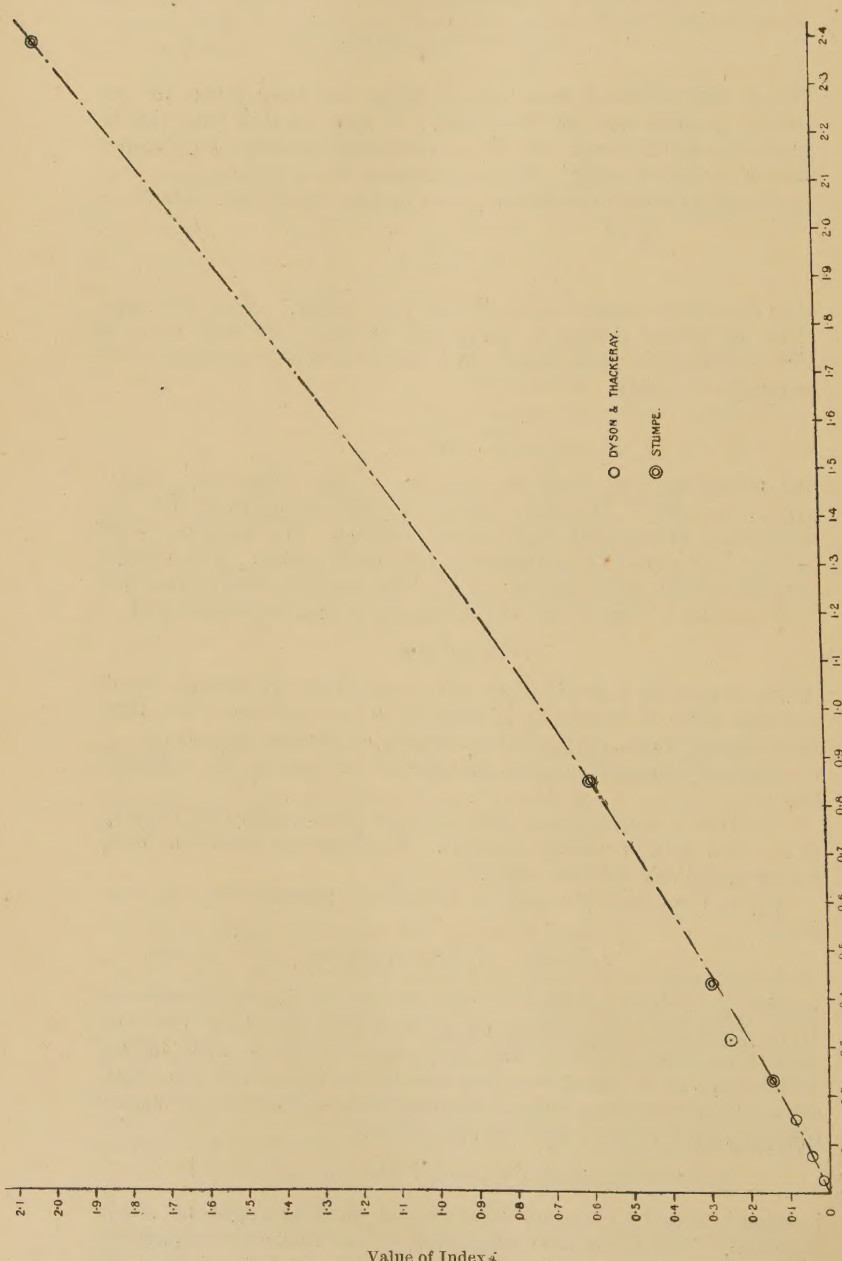
(ii) π observed is largely a differential parallax $= \pi_2 - \pi_1$, say. Hence

$$\sigma_\pi{}^2 = \sigma_{\pi_2}{}^2 + \sigma_{\pi_1}{}^2 - 2\sigma_{\pi_1}\sigma_{\pi_2}r_{12},$$

where r_{12} is the correlation between the component parallaxes. If r_{12} were zero, then again σ_π is sensibly larger than the true value σ_{π_2} or σ_{π_1}; but if the component parallaxes were highly correlated, *i.e.* if the two stars observed were about the same distance from the Sun, notwithstanding their differences of magnitude or proper motion, then we should have

$$\sigma_\pi{}^2 \text{ possibly nearer } (\sigma_{\pi_2} - \sigma_{\pi_1})^2 \text{ than to } \sigma_{\pi_1}{}^2 + \sigma_{\pi_2}{}^2;$$

in other words the observed variability of parallax might be much in defect. This is the only suggestion we can make to account for σ_π being sensibly less than the true value of the sample. It


DYSON & THACKERAY.
STUMPE.
Value of Index i.
Value of Proper Motion.

involves the assumption that near stars on which the differential parallax has been worked are really near each other.

(iii) If we study the graph (Diagram VI.) of the Dyson-Thackeray and Stumpe observations, we see that the three points of the former investigators and the first of the latter fall nearly on a straight line of slope about ·5. If we were to give m its initial value = ·45, say, an increase of speed in the Sun to about 20 miles per second would render the two results accordant. The assumption here made is that the stars with a large proper motion belong possibly to a different system to those with the smallest proper motions, and ought not to be used to determine m.

(iv) Lastly, it is possible that among the stars used by Dyson and Thackeray and by Oscar Stumpe, there are many with considerably higher parallaxes than appear in the small samples for which parallax has at present been determined, and that thus the variability of parallax will ultimately be found to exceed the values so far determined. This must denote the existence of a *considerable* number of really large parallaxes hitherto undiscovered, for we must not forget that the present values of parallax have, in all probability, a considerably exaggerated variability due to observational error.

From the statistical standpoint, therefore, the determination of considerable numbers of additional parallaxes seems to be a rather pressing problem. It is not unreasonable to suggest that there is something that needs reconciling in the values determined for the index i and the value usually assumed for the Sun's motion in space.

(8) *Correlations with Proper Motion.* — Sufficient material exists for finding the association, if any, between proper motion and colour, but we have not yet had the opportunity of dealing with it. The fact that the correlation ratio for spectral classes is as high as ·39 would indicate that colour and proper motion may well be sensibly related. Magnitude and parallax correlations with proper motion have been dealt with above. Accordingly, we have approximately the following system :—

Proper Motion and Parallax	·39
Proper Motion and Spectral Class . .	·36
Proper Motion and Magnitude	·35

(9) *Conclusions.* — While we are fully aware how badly the mere statistician may stumble in dealing with astronomical data, we still think that the general relationships shown by the statistical correlation constants may be of value to astronomers. They serve to indicate the directions in which closer relationships may be found, and where, possibly, more effective classifications may be made. The values given in the accompanying general scheme are certainly not final, but we do not think that they give at all misleading values, or values really far from the truth. There remains much to be done, and the scheme indicates some of the values which

yet remain to be found. While parallax touches one element only of position, direction as indicated by the usual stellar co-ordinates is a second; this "position" in the narrower sense has not been discussed in the present paper. We have purposely included it in the table, to indicate that we have not overlooked those position correlation problems to which the astronomer is now turning, and to which approach is possible from more than one direction. We hope later to deal with the question of correlation of stellar characters and position. Taken as a whole, we are, we think, compelled to conclude that the associations between parallax, proper motion, and magnitude are considerably less than we should anticipate if we hypothecated any approach to a uniform distribution of stars with a system of random velocities throughout space. The existence of correlations between colour and spectral class, not only with magnitude, but with parallax and proper motion, suggests, if it does not demonstrate, that chemical constitution and luminosity are dependent in some manner not only on spatial distribution, but on velocity in space.

TABLE XVII.

Correlation of Stellar Characters.

	Colour.	Spectral Class.	Magnitude.	Parallax.	Proper Motion.	Position.
Colour	1·00	·71	·30	?	?	?
Spectral class	·71	1·00	·69 (·43, ·54*)	·36	·36	?
Magnitude	·30	·69 (·43, ·54*)	1·00	·30	·35	?
Parallax	?	·36	·30	1·00	·39	?
Proper motion	?	·36	·35	·39	1·00	?
Position	?	?	?	?	?	1·00

* According to Pickering's and Lockyer's classifications respectively.

Erratum in Annual Report.

Vol. lxviii. p. 298, line 5, *for* 0″·1 *read* 0″·01.